SCIENCE

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*HUXLEY AND HIS WORK.**

I.

THE history of scientific progress has been marked by a few periods of intellectual fermentation when great bounds have been taken forwards and a complete revolution ensued. Very few have been such, but in one the name of Huxley must be ever conspicuous. It was as a lieutenant of the organizer of that revolution that he appeared, but unquestionably without him it would have been long delayed, and it was through his brilliant powers of exposition that the peoples of the English speaking lineage soon learned to understand, to some extent, what evolution was and, learning, to accept it.

On the 4th of May, 1825, was born the infant Huxley, in due course christened Thomas Henry. "It was," Huxley himself has remarked, "a curious chance that my parents should have fixed for my usual denomination upon the name of that particular apostle with whom I have always felt most sympathy." In his physical and mental peculiarities, he was completely the 'son of his mother,' whose most distinguishing characteristic was 'rapidity of thought;' that characteristic Huxley claimed to have been passed on to him 'in full strength,' and to have often 'stood him in good stead,' and to it he was

* A memorial address given on January 14th before the Scientific Societies of Washington.

undoubtedly indebted for success in the many intellectual duels he was destined to be engaged in. His 'regular school training was of the briefest,' and he has expressed a very poor opinion of it. His early inclination was to be a mechanical engineer, but he was put to a brother-in-law to study medicine. The only part of his professional course which really interested him was physiology, which he has defined as 'the mechanical engineering of living machines.' The only instruction from which he thought he ever obtained the proper effect of education was that received from Mr. Wharton Jones, who was the lecturer on physiology at the Charing Cross School of Medicine. At Mr. Jones' suggestion, in 1845, Huxley communicated to the *Medical Gazette* (p. 1340) his first paper 'On a hitherto undescribed structure in the human hair sheath.' Two years later he contributed to the British Association for the Advancement of Science the first paper generally attributed to him—'Examination of the corpuscles of the blood of Amphioxus.' (Abstracts, p. 95.) In 1845 he passed the first M. B. examination at the London University. Soon afterwards he was admitted into the medical service of the Navy and was, after some waiting, assigned to the Rattlesnake, and for four years (1846–50) served on her during her exploration of the Australasian seas; he was, he supposed, among the last voyagers 'to whom it could be possible to meet with people who knew nothing of firearms—as [they] did on the south coast of New Guinea.'

While on board Huxley zealously prosecuted zoölogical investigations and in 1849 and 1850 sent records of observations, especially on cœlenterates, in papers which were published in the 'Philosophical Transactions' and 'Annals of Natural History.' Most important of all was a monograph on the Oceanic Hydrozoa published by the Ray Society. It is amusing to find that while in Sydney he was impressed by MacLeay and led to believe that "there is a great law hidden in the 'Circular system' if we could but get at it, perhaps in Quinarianism too,"* but sober sense doubtless soon came to the rescue and he appears to have been never otherwise touched by the strange monomania that had been epidemic in England during the previous quarter century. In 1851 he became a F. R. S. He continued in the navy three years after his return, but in 1853 resigned when ordered to sea again.

In 1853 Huxley and Tyndall became candidates for professorships in the University of Toronto, but that University preferred others for the vacant places and thus missed the opportunity of an age. In 1854 Huxley was appointed to the post of paleontologist and lecturer on natural history in the School of Mines which he held for the next thirty-one years. In the same year he became Fullerian Professor to the Royal Institution. "The first important audience [he] ever addressed was at the Royal Institution." In 1862 he served as President of the Biological Section, and in 1870 of the 'British Association for the Advancement of Science' itself, in 1869 and 1870 of the Geological and Ethnological Societies, and in 1883 to 1885 of the Royal Society. He was Inspector of Salmon Fisheries from 1881 to 1885.

In 1876 he visited the United States and delivered an address at the opening of the Johns Hopkins University.

In 1885 failing health and desire for freedom led him to retire from most of his offices and thenceforth he devoted himself chiefly to literary work rather than to scientific investigation. On the accession of Lord Salisbury to the Premiership in 1892, Huxley was made Privy Counsellor, and with it came the title of Right Honorable, by which he was later styled. In the last years of life he resided at Hodeslea, East-

* Ann. Mag. Nat. Hist. (2), VI., p. 67.

bourne, and after a long illness ('complication following influenza'*) died there on the 29th of June, 1895.

Such were the principal episodes in the life of Huxley. Many more details may be found in the numerous periodicals of the day and in some of them are depicted various phases of his character and labors. The short time that is at our disposal to-night may be most profitably and entertainingly utilized in reviewing his feats as a warrior of science and estimating the measure of influence he exercised in diverting human thought from the ruts in which it had moved for centuries and directing it into a highway where increasing light from different sides could guide the wayfarer. Although this period of warfare was at its height not farther back than the early afternoon of the present century, and some of us here assembled joined in the fray, to the younger naturalists it is an unknown past except through history, and to some of us who were of it, it is so strange as to recur to us rather as a dream than as a realized passage in actual life.

II.

Doubtless man, almost from the moment of his acquisition of those characters which distinguish him as representative of the genus Homo, had wondered and speculated as to how he came into being and how the animals assembled round him had sprung into existence. Those early concepts must have been strange indeed, but were doubtless transmitted from mother to child, only with some eccentricities lopped off with advancing intelligence. Gradually, among peoples of the Aryan stock at least, they crystallized into a doctrine that in the beginning there was chaos, that the three elements of air, water and earth were differentiated, and that animals were successively created to occupy the spaces. Such were the views of the old oriental cosmologists and such of the later Romans as epitomized in Ovid's verse. These ideas were long regnant and naturalists embodied some in their schemes, most accepting the idea that animals may have been created in pairs, but a few (such as Agassiz) urging that they must have been created in communities approximating to those still found. There were very few to dissent from these views of specific creation, and those few had little influence on the popular beliefs. But as the present century advanced, curious men delved into all the mysteries of nature; the sciences of morphology, physiology, histology, embryology, geology and zoögeography came into being, and facts were marshalled from every side that militated against the old conceptions. Even when these sciences were inchoate, or new born, sagacious men had perceived the drift of the facts and anticipated induction by the formulation of hypotheses of evolution, but the hypotheses were too crude to ensure acceptance. Meanwhile, however, the facts accumulated, and in 1859 a factor determining the course of development of species was appreciated by Darwin and Wallace, and soon applied to a wide range of facts in the former's 'Origin of Species by means of Natural Selection.'

Darwin's work at once aroused great popular interest, but it was too diffuse and the intellectual pabulum it contained was too strong and indigestible for ordinary readers, and it is probable that the general acceptance of the Darwinian form of evolution would have been delayed much longer than it was had it not been for the excursions from the scientific fold into the popular arena by one having the confidence of the former and the ear of the latter, as did Huxley.

Scarcely had Darwin's work come from the press when Huxley commenced his missionary work. Almost exceptional among numerous reviews, remarkable chiefly for

*Lancet, July 6, p. 64, 65.

crudity, ignorance and arrogance, was one that appeared in the great daily organ of English opinion—*The Times*—marked by superior knowledge, acuteness of argumentation, and terse and vigorous style. This review, which attracted general attention, was acknowledged later by Huxley. Lectures and addresses before popular audiences and even to those distinctively claiming to be 'workingmen' followed, and these were published or supplemented by publication in various forms. Answers, critiques and other articles in reply came out in rapid succession, and loud clamor was made that Huxley was an infidel and a very bad man, and that he falsified and misrepresented in a most villainous manner.

A memorable occasion was the meeting of the British Association for the Advancement of Science in the year 1860, following the publication of the Origin of Species. A discussion of the subject was precipitated by the presentation of a communication by our own Draper, 'On the Intellectual Development of Europe with reference to the views of Mr. Darwin and others, that the progression of organisms is determined by law.' The Rev. Mr. Creswell and the Rev. Dr. Wilberforce, Bishop of Oxford, followed in opposition, and they were answered by Huxley. The scene has lately been redescribed by a great physiologist and friend of Huxley, who is one of the few witnesses who now remain. "The room was crowded, though it was Saturday, and the meeting was excited. The bishop had spoken; cheered loudly from time to time during his speech, he sat down amid rapturous applause, ladies waving their handkerchiefs with great enthusiasm; and in almost dead silence, broken merely by greetings which, coming only from the few who knew, seemed as nothing, Huxley, then well-nigh unknown outside the narrow circle of scientific workers, began his reply. A cheer, chiefly from a knot of young men in the audience, hearty but seeming scant through the fewness of those who gave it, and almost angrily resented by some, welcomed the first point made. Then as, slowly and measuredly at first, more quickly and with more vigor later, stroke followed stroke, the circle of cheers grew wider and yet wider, until the speaker's last words were crowned with an applause falling not far short of, indeed equalling that which had gone before, an applause hearty and genuine in its recognition that a strong man had arisen among the biologists of England."

The versatile bishop indulged in the argumentum ad hominem so very trite and familiar to us all (Who has not heard it?): he would like 'to hear from Mr. Huxley whether it was by his grandfather's or grandmother's side that he was related to an ape.'

Huxley replied and answered: "I asserted, and I repeat, that a man would have no reason to be ashamed of having an ape for a grandfather. If there were an ancestor whom I should feel shame in recalling, it would be a man, a man of restless and versatile intellect who, not content with an equivocal success in his own sphere of activity, plunges into scientific questions with which he has no real acquaintance, only to obscure them by an aimless rhetoric and distract the attention of his hearers from the real point at issue by eloquent digressions and skilled appeals to religious prejudice."

The arguments adduced against evolution during those days were sometimes very comical, and the confident air of the upholder of the ancient views and the assurance with which he claimed that his position was fixed and that the burden of proof rested entirely upon the advocate of the opposite view, were very amusing. It was urged that no one had ever *seen* one species turn into another! Had any one ever *seen* any animal made? Could any one really

conceive of any animal being actually made? Did an omnipotent Creator actually take the 'dust of the ground' and mould it into animal shape and then breathe into its nostrils 'the breath of life.' 'Did infinitesimal atoms flash into living tissues.' Certainly no physiologist with a competent knowledge of histology could believe in any such mode of creation! On the other hand, every one that could exercise the necessary skill could follow the evolution of an animal from an undifferentiated protoplasmic mass into a perfect animal. A clutch of eggs could be successively taken from a mother hen or a hatching oven, and day after day the actual evolution of the undifferentiated matter into derivative functional parts could be followed. That which is true of the hen is true of man, only in the latter case it is more difficult to obtain the requisite material, and greater skill to use it is requisite. Compare the embryos developing in the hen and human eggs and at first no difference except size and environment can be perceived. Compare them in successive stages, and adult animals more or less parallel to some early stages may be found still living or entombed in earlier formations of the earth in fossilized form.

It was argued that no one had ever seen one species turn into another! But is it not a matter of historical evidence that many breeds of domestic animals have actually been developed by the agency of man and propagate their kind? And how are such breeds distinguished from species except by the fact that we know their origin, and that they have come into prominence through selection by man rather than by Nature? Interbreeding is no criterion.

But it is unnecessary to go into details, and these hints are offered only because their bearings on the subject were so generally overlooked by those who opposed evolution. One opponent, so eminent as to be styled the 'Pope' of a great Protestant Church, published a work against evolution, largely based on the contention that the existence of the eye, except through direct creation, was inconceivable! Yet this very evolution of the eye from simple protoplasm could have been witnessed at any time with little trouble in the hen's egg! Is evolution through great reaches of time more inconceivable than actual evolution capable of daily observation?

Well and skillfully did Huxley meet the arguments against evolution. Even most of the old naturalists sooner or later recognized the force of the arguments for, and the weakness of those against, evolution. Those who did not in time gave up the contest with their lives. The young who later entered into the field of investigation have done so as evolutionists.

It is interesting to recall that the illustrious American (Prof. Dana) who recently departed so full of years and honors, and of whom you have heard from a former speaker (Major Powell) to-night, at length, in the full maturity of his intellect, accepted unconditionally the doctrine of evolution and dexterously applied it in his last great work.

III.

Darwin, in his Origin of Species, had refrained from direct allusion to man in connection with evolution and many casual readers were doubtless left in uncertainty as to his ideas on the subject. Naturally, the scientific man recognized that the origin of his kind from a primate stock followed, and believed that Darwin's reticence was probably due to a desire to disturb popular beliefs as little as possible. When we recall what strange views were held respecting man's origin and relations we can understand how the unlearned could easily fail to recognize that man must follow in the chain of his fellow creatures. (We preserve creature still as a reminiscence of ancient belief,

but without the primitive conception attached to the word.)

Man was claimed as a being isolated from animals generally, and naturalists of acknowledged reputation, and one or two of great fame, more or less completely differentiated him from the rest of the animal kingdom and even from the animal kingdom itself.

As long as the isolation of man from the animal kingdom, or from the greater part, was based on metaphysical or psychological ideas, the naturalist perhaps had no cause of quarrel, although he might wonder why a morphologist should stray so far from the field of observation. But when naturalists confused morphological and psychological data, he had reason to protest. This confusion was effected by one of great eminence. There was no naturalist in Britain about the middle of the century who enjoyed a reputation equal to that of Richard Owen. An anatomist of preëminent skill and extraordinary industry, his merits had been appreciated by the entire world. An opinion of his had a weight accorded to no others. Consequently a new classification of the mammals, published by him in 1857, soon became popular. This classification was founded on alleged characters of the brain and on successive phases of increase in the cerebrum. Man was isolated not only as the representative of a family, but of an order and subclass.

According to Owen, "in Man the brain presents an ascensive step in development, higher and more strongly marked than that by which the preceding subclass was distinguished from the one below it. Not only do the cerebral hemispheres overlap the olfactory lobes and cerebellum, but they extend in advance of the one and further back than the other. Their posterior development is so marked that anatomists have assigned to that part the character of a third lobe; it is peculiar to the genus Homo and equally peculiar is the 'posterior horn of the lateral ventricle,' and the 'hippocampus minor,' which characterize the hind lobe of each hemisphere. The superficial grey matter of the cerebrum, through the number and depth of the convolutions, attains its maximum of extent in Man. Peculiar mental powers are associated with this highest form of brain, and their consequences wonderfully illustrate the value of the cerebral character."

The views thus expressed by Owen were reiterated on various occasions, but many anatomists dissented from them and the rumbling of a future storm was betokened. At last the stormcloud broke and Owen was overwhelmed. At a great popular assemblage at Oxford, on the occasion of the meeting of the British Association for the Advancement of Science, Owen once more urged his contention of the cerebral characteristics of man and maintained this wide difference from the apes.

Huxley immediately rose and, with that cogency of reasoning which characterized him, proceded to divest the subject of the sophistries in which it had been enveloped. "The question," he said, "appeared to him in no way to represent the real nature of the problem under discussion. He would therefore put that problem in another way. The question was partly one of facts and partly one of reasoning. The question of fact was, What are the structural differences between man and the highest apes?—the question of reasoning, What is the systematic value of those differences? Several years ago Prof. Owen had made three distinct assertions respecting the differences which obtained between the brain of man and that of the highest apes. He asserted that three structures were 'peculiar to and characteristic' of man's brain—these being the 'posterior lobe,' the 'posterior cornu,' and the 'hippocampus minor.' In a controversy

which had lasted for some years, Prof. Owen had not qualified these assertions, but had repeatedly reiterated them. He (Prof. Huxley), on the other hand, had controverted these statements; and affirmed, on the contrary, that the three structures mentioned not only exist, but are often better developed than in man, in all the higher apes. He (Prof. Huxley) now appealed to the anatomists present in the section whether the universal voice of Continental and British anatomists had not entirely borne out his statements and refuted those of Prof. Owen. Prof. Huxley discussed the relations of the foot of man with those of the apes, and showed that the same argument could be based upon them as on the brain; that argument being that the structural differences between man and the highest ape are of the same order, and only slightly different in degree from those which separate the apes one from another. In conclusion he expressed his opinion of the futility of discussions like the present. In his opinion the differences between man and the lower animals are not to be expressed by his toes or his brain, but are moral and intellectual."

The appeal to anatomists was answered on the spot. The foremost anatomists of England there present (Rolleston and Flower) successively rose and endorsed the affirmations of Huxley. Not one supported Owen and, brilliant as his attainments were, his want of candor entailed on him the loss of his eminent place, and Huxley took the vacated throne. But the contest that resulted in Owen's overthrow was of great service, for in the chief centers of civilization anatomists eagerly investigated the question at issue, and the consequence was that in a few years more material had been collected and studied than under ordinary conditions would have been done in five times the period. Unlike other battles, one in scientific warfare is almost always advantageous to the general cause, whatever it may be to a party.

IV.

The first important memoir by Huxley was written in his twenty-third year 'On the Anatomy and the Affinities of the Family of the Medusæ' (Phil. Trans., 1849, pp. 413–434, pl. 37–39), and contained the germ of a fundamental generalization. He therein laid 'particular stress upon the composition of [' the stomach '] and other organs of the Medusæ out of *two distinct membranes*, as [he says] I believe that is one of the essential peculiarities of their structure, and that a knowledge of the fact is of great importance in investigating their homologies. I will [he continues] call these two membranes as such and independently of any modification into particular organs, 'foundation membranes' (p. 414). In his summary (p. 425) he also formulates 'that a Medusa consists essentially of two membranes, inclosing a a variously-shaped cavity, inasmuch as its various organs are so composed.'

I have thus given Huxley's own words inasmuch as Prof. Haeckel has asserted that Huxley therein "directed attention to the very important point that the body of these animals is constructed of two cell-layers—of the Ectoderm and the Endoderm—and that these, physiologically and morphologically, may be compared to the two germinal layers of the higher animals" (Nature, 1874), and Prof. Kowalevsky has also claimed that Huxley "founded modern embryology by demonstrating the homology of the germinal layers of Vertebrates with the ectoderm and endoderm of Cœlenterates" (Nature, Oct. 31, 1895, p. 651).

In all candor I must confess that, important as the generalization of Huxley for the Medusae was, it was only applied by him to the Medusae, and was not *necessarily* extensible with the homologies indicated, but it was pregnant with suggestiveness and to

that extent may have led to the wider generalization that followed. Let all possible credit then be assigned to it.

The classification of animals generally adopted, and in this country especially, up to at least the early years of the present half century, was based on what was called plan or type and was mainly due to Cuvier. According to this school there were four 'great fundamental divisions of the animal kingdom,' and these were 'founded upon distinct plans of structure, cast, as it were, into distinct moulds or forms.' The term generally used to designate this category was branch or subkingdom and the subkingdoms themselves were named Vertebrates, Mollusks, Articulates and Radiates. Various modifications of this system and more subkingdoms were recognized by many zoölogists, but the one specially mentioned was in very general use in the United States because favored by Agassiz, who then enjoyed a great reputation. Almost all naturalists of other countries, and many of this, recognized the distinctness, as subkingdoms or branches, of the Protozoans and Cœlenterates. But Huxley, in 1876, went still further and segregated all animals primarily under two great divisions based on their intimate structure, accepting for one the old name, Protozoa, and for the other Haeckel's name, Metazoa.

"Among those animals which are lowest in the scale of organization there is a large assemblage, which either present no differentiation of the protoplasm of the body into structural elements; or, if they possess one or more nuclei, or even exhibit distinct cells, these cells do not become metamorphosed into tissues—are not histogenetic. In all other animals, the first stage of development is the differentiation of the vitellus into division-masses, or blastomeres, which become converted into cells, and are eventually metamorphosed into the elements of the tissues. For the former the name Protozoa may be retained; the latter are coextensive with the Metazoa of Haeckel."

While not exactly original with Huxley, the recognition of these two great categories of the animal kingdom was hastened among naturalists, and found place in most of the works by men of authority that followed. That such recognition greatly facilitates morphological concepts is certain. But most of the further new features of this classification have not received the approbation of naturalists generally. And here it may be admitted that Huxley was rather a morphologist in a narrow sense, or anatomist rather than a systematist of greatly superior excellence. Unquestionably he did much excellent work in systematic zoölogy, but the direct subject of investigation was perhaps treated from too special a standpoint, and sometimes without an attempt to coördinate it with the results in other fields, or to measure by some given standard. He was indeed a great artist, but he used his powers chiefly to sketch the outlines of a picture of nature. This was done with the bold and vigorous hand of a master, but his productions were deficient in details and finish and were sometimes imperfect on account of inattention to perspective and perhaps deliberate neglect of the niceties of nomenclature. (And lest I may be misunderstood, let me here explain that by systematic zoölogy I mean the expression of *all* the facts of structure in a form to best represent the values of the differences as well as resemblances of all the constituents and parts of the entire organization, from the cells to the perfected organs and the body as a whole.) For example, he separated Amphibians from Reptiles and combined them with Fishes, and yet under the last name comprised the Leptocardians and Marsipobranchs, and to his influence is doubtless due to a large extent the persistence of English (but not American) naturalists in a combi-

nation which is elsewhere regarded as contradicted by all sound morphological doctrine.* The value of the characters distinctive of the Rhynchocephalian reptiles and their consequent significance for taxonomy and paleontology were also denied by him. Nevertheless, even his negative position was of use in that it incited investigation. The numerous memoirs on the anatomy and characteristics of various groups of animals, too, were always replete with new facts and the hints were almost always sagacious, even if not always in exactly the right direction.

I am inclined to credit mainly to his sagacity the early appreciation of the affinity of the *Neoceratodus* of Australia to the mesozoic Ceratodontids with all the far-reaching consequences that appreciation involved. It was in 1870 that the living Ceratodontid was introduced to the scientific world as *Ceratodus Forsteri*, and thus generically associated with the mesozoic fishes. How did Krefft (or Clarke) get the idea of this association of a living fish with some known only from fossil teeth referred by Agassiz to the same family as the Cestraciont sharks? In 1861 Huxley published a 'Preliminary Essay upon the Systematic Arrangement of the Fishes of the Devonian Epoch,' and therein suggested that *Ceratodus* was a Ctenodipterine fish and ranged it (with a mark of interrogation) by the side of *Dipterus*. He also drew 'attention to the many and singular relations which obtain between that wonderful and apparently isolated fish, *Lepidosiren*,' and the Ctenodipterine fishes. (The exact truth was not discovered, but was approximated.) Is it not probable that this memoir was known to Clarke, who claimed to have suggested to Krefft the systematic relations of newly discovered Australian dipnoan? It was creditable to both Clarke and Krefft that they did recognize this relationship and profited by their bibliographical knowledge, but it is doubtful whether they would have been able to make the identification or appreciate the importance of the discovery had not Huxley prepared partly the way. By this discovery, our acquaintance with the ichthyic faunas of both the present and past was almost revolutionized.

* The great English morphologists (such as Balfour and Ray Lankester) and A. Smith Woodward among systematic ichthyologists have recognized the heterogeneity of the old class of fishes.

Among the most important results of Huxley's investigations were the discovery and approximately correct recognition of the nature of the 'peculiar gelatinous bodies' found in all the seas, whether extra-tropical or tropical, through which the 'Rattlesnake' sailed, and which were named *Thalassicola*, precursors of radiolarian hosts afterwards to be brought to light; the appreciation of the closeness of the relations between birds and reptiles, the destruction of the old basis for the classification of birds, the recognition that mammals may have originated from a low type of Vertebrates and even the Amphibians, and the perception of the comparative affinities of the southern forms of Astacoidean crustaceans and their contrast as a group with the forms of the northern hemisphere. I must resist the temptation to further enumerate the great naturalist's discoveries and generalizations, but finally let me add that not the least of his services to science was destructiveness in the death-blow he gave to the vertebral theory of the skull at one time so generally accepted in England and this country.

V.

While the contest between the old and new schools of biological philosophy was at its height, the former was almost entirely supported by the religious element and bitter were the invectives against evolution. The opposition was almost solely based on

the ground that the doctrine was in opposition to revealed religion. The naturally combative disposition of Huxley was much aroused by this opposition, and the antagonism early engendered was kept aglow during his entire life. Meanwhile it had been discovered by many of the more sagacious and learned clergymen that there was no real antagonism between the Scriptural account of Creation and evolution, but that the two could be perfectly reconciled. The reconciliation had been effected between Genesis and astronomy and between Genesis and geology, and was continued on the same lines for Genesis and evolution. But Huxley would have none of it. He gave expression to his convictions in the following words:

"For more than a thousand years, the great majority of the most highly civilized and instructed nations in the world have confidently believed and passionately maintained that certain writings, which they entitle sacred, occupy a unique position in literature, in that they possess an authority, different in kind, and immeasurably superior in weight, to that of all other books. Age after age, they have held it to be an indisputable truth that, whoever may be the ostensible writers of the Jewish, Christian, and Mahometan Scriptures, God Himself is their real author; and, since one of the attributes of the Deity excludes the possibility of error and—at least in relation to this particular matter—of wilful deception, they have drawn the logical conclusion that the denier of the accuracy of any statement, the questioner of the binding force of any command, to be found in these documents is not merely a fool, but a blasphemer. From the point of view of mere reason he grossly blunders; from that of religion he grievously sins.

"But, if this dogma of Rabbinical invention is well founded; if, for example, every word in our Bible has been dictated by the Deity; or even if it be held to be the Divine purpose that every proposition should be understood by the hearer or reader in the plain sense of the words employed (and it seems impossible to reconcile the Divine attribute of truthfulness with any other intention), a serious strain upon faith must arise. Moreover, experience has proved that the severity of this strain tends to increase, and in an even more rapid ratio, with the growth in intelligence of mankind and with the enlargement of the sphere of assured knowledge among them.

"It is becoming, if it has not become, impossible for men of clear intellect and adequate instruction to believe, and it has ceased, or is ceasing, to be possible for such men honestly to say they believe that the universe came into being in the fashion described in the first chapter of Genesis; or to accept, as a literal truth, the story of the making of woman with the account of the catastrophe which followed hard upon it, in the second chapter; or to admit that the earth was repeopled with terrestrial inhabitants by migration from Armenia to Kurdistan, little more than 4,000 years ago, which is implied in the eighth chapter; or finally, to shape their conduct in accordance with the conviction that the world is haunted by innumerable demons, who take possession of men and may be driven out of them by exorcistic adjurations, which pervades the Gospels."

So far even Huxley was not in disagreement with some of the most eminent and learned of theologians. Those of you who are interested will be able to recall utterances of enlightened clergymen which would differ from Huxley's only in the absence of the leaven of sarcasm that permeates his lines. At a late Congress of the Church of England, held at Norwich, the Rev. Canon and Professor Bonney gave voice to words that convey the same ideas as Huxley's.

"I cannot deny," he said, "that the increase of scientific knowledge has deprived

parts of the earlier books of the Bible of the historical value which was generally attributed to them by our forefathers. The story of the Creation in Genesis, unless we play fast and loose either with words or with science, cannot be brought into harmony with what we have learned from geology. Its ethnological statements are imperfect, if not sometimes inaccurate. The stories of the Flood and of the Tower of Babel are incredible in their present form. Some historical element may underlie many of the traditions in the first eleven chapters of that book, but this we cannot hope to recover."

But Huxley was not content to deny any authority to the Scriptural basis of most of the religions of Europe and America. He denied that there was any means of knowing what the future had in store. He did not deny that there was a heaven or a hell; he did not deny that in a future world man might continue in a sublimated state, and might be punished for his misdeeds or rewarded for the good deeds he had performed and for good thoughts on earth. He did not venture to express any opinion on the subject for the reason that he had no data to base an opinion upon. He called himself an agnostic and the attitude he assumed was agnosticism.

This term agnostic, we are told by Mr. R. H. Hutton, was suggested by Prof. Huxley at a party held previous to the formation of the now defunct Metaphysical Society, at Mr. James Knowles' house on Clapham Common, one evening in 1869, and was suggested by St. Paul's mention of the altar to the unknown God—'Αγνώστῳ Θεῷ.

But Huxley has explained that he assumed this term in contradistinction to the gnostic of old. The gnostic claimed to know what in the nature of things is unknowable, and as Huxley found himself with an exactly opposite mental status, he coined a word to express that antithetical state—agnostic.

I have done all I conceive to be necessary in giving this statement of Huxley's attitude. Whether he was right or wrong, each one must judge for himself or herself. Believing as he did, on a bed of prolonged illness he resignedly awaited the inevitable, and desired that his sentiments reflected in verse by his wife should be engraved on his tomb.

"And if there be no meeting past the grave,
If all is darkness, silence, yet 'tis rest.
Be not afraid, ye waiting hearts that weep
For God 'still giveth his beloved sleep,'
And if an endless sleep he wills—so best."

THEO. GILL.

CERTITUDES AND ILLUSIONS.

CHUAR'S ILLUSION.

IN the fall of 1880 I was encamped on the Kaibab plateau at the edge of the forest above the canyon gorge of a little stream. White men and Indians composed the party with me. Our task was to make a trail down this side canyon into the depths of the Grand Canyon of the Colorado. While in camp after the day's work was done, both Indians and white men engaged in throwing stones across the little canyon, which was many hundreds of feet in depth. The distance from the brink of the wall on which we were camped to the brink of the opposite wall seemed not very great, yet no man could throw a stone across the chasm, though Chuar, the Indian chief, could strike the opposite wall very near its brink. The stones thrown by others fell into the depths of the canyon. I discussed these feats with Chuar and led him on to an explanation of gravity. Now Chuar believed that he could throw a stone much farther along the level of the plateau than over the canyon. His first illusion was thus one very common among mountain travelers—an underestimate of the distance of towering and massive rocks when the eye has no intervening objects to divide the space into parts as measures of the whole.

I did not venture to correct Chuar's judgment, but simply sought to discover his method of reasoning. As our conversation proceeded he explained to me that the stone could not go far over the canyon, for it was so deep that it would make the stone fall before reaching the opposite bank; and he explained to me with great care that the hollow or empty space pulled the stone down. He discoursed on this point at length, and illustrated it in many ways: "If you stand on the edge of the cliff you are likely to fall; the hollow pulls you down, so that you are compelled to brace yourself against the force and lean back. Any one can make such an experiment and see that the void pulls him down. If you climb a tree the higher you reach the harder the pull; if you are at the very top of a tall pine you must cling with your might lest the void below pull you off."

Thus my dusky philosopher interpreted a subjective fear of falling as an objective force; but more, he reified void and imputed to it the force of pull. I afterward found these ideas common among other wise men of the dusky race, and once held a similar conversation with an Indian of the Wintun on Mount Shasta, the sheen of whose snow-clad summit seems almost to merge into the firmament. On these dizzy heights my Wintun friend expounded the same philosophy of gravity.

Now in the language of Chuar's people, a wise man is said to be a traveler, for such is the metaphor by which they express great wisdom, as they suppose that a man must learn by journeying much. So in the moonlight of the last evening's sojourn in the camp on the brink of the canyon, I told Chuar that he was a great traveler, and that I knew of two other great travelers among the white men of the East, one by the name of Hegel and another by the name of Spencer, and that I should ever remember these three wise men, Chuar, Hegel and Spencer, who spoke like words of wisdom, for it passed through my mind that all three of these philosophers had reified void and founded a philosophy thereon.

In the history of philosophy an illusion is discovered concerning matter and each of the constituents or categories of matter, which are number, extension, motion, duration and judgment; and as bodies are related elements of matter, relation itself comes to be the object of illusion. Matter is the substrate of all bodies ; bodies thus have a substrate, and the illusion of matter arises from supposing that matter, which is the substrate, has also its substrate, which is sometimes called essence. Classes are orders of number ; the illusion of number relates to class or kind, and this is also usually called essence. Extensions combined have figure and structure, which produce form, and the illusion of extension is an illusion in relation to forms which are derived from extensions, and is called space. Motions through collisions are forces, and the illusion relating to motion is also called force. Duration is persistence and change, which give rise to time, and the illusion of duration is called time. Judgment is consciousness and inference, which give rise to comprehension of ideas, and the illusion of idea is called ghost. Bodies are related to one another, hence numbers, extensions, motions, durations and judgments are related. Certain of the relations of these things are called cause, and the illusion of relation also is called cause.

Now it must be clearly understood that the terms substrate, essence, space, force, time, ghost and cause refer sometimes to real things, as when properly used in science, and sometimes to illusions, when they are improperly used, as they often are in metaphysics ; but usually the word ghost is now used only in reference to an illusion,

and this is the sole case where we have a term for an illusion which is commonly understood in that sense, but the term spirit is used in both senses, for the certitude and for the illusion.

The seven illusions here enumerated are perhaps the most fundamental and far-reaching of the vast multitude of illusions which appear in the history of error. The words substrate, essence, space, force, time, ghost and cause are terms of universal use and their synonyms appear in all civilized languages, and perhaps in all lower languages. They have always stood for certitudes and illusions; here they require definitions both as certitudes and as illusions, in so far as we are able to define them.

SUBSTRATE.

Substrate is matter, matter is the substrate of all bodies. Essence is any collocation of units into a unit of a higher order which makes it a kind or one of a class. Space is any extension or any collocation of extensions; force is any collocation of motions that are related by collisions; time is any duration or collocation of durations; mind or spirit or ghost is any cognition or collocation of cognitions; cause is any related antecedent or collocation of such antecedents of a change. Such are the fundamental meanings of the words when used to designate realities. We shall hereafter see what they mean when they are used to designate illusions. Matter is the substrate of body and has no substrate for itself. All matter has four factors or constituents, number, extension, motion and duration, and some matter at least has a fifth factor, namely judgment. Matter is not a substrate for these factors, but exists in these constituents which are never dissociated, but constitute matter, or are the moments of matter; and this matter is the substrate of all bodies.

ESSENCE.

The term essence as used in philosophy is employed in a double manner and is thus often ambiguous. It is sometimes used as a synonym for substrate of matter, at other times it is used to designate the occult substrate of class. In this latter sense it is here used. Essence, then, is the number essential to make an order or kind of a class. As the whole number is essential, every one is essential; they are severally and conjointly essential, so that it is possible correctly to speak of them all as being essential and to speak of every one severally as being essential. All of the particles which make up a body are conjointly and severally essential to that body, and the essence of a body is the hierarchy of particles of which it is composed. The term essence, therefore, is a general term or pronoun for all collocations of number, and its special meaning is derived from the context. As an illusion, essence is the name of an unknown something which produces a kind or class, and is a property of an unknown or unknowable substrate of matter.

If, as the chemist believes, with much good reason, the ultimate chemical particles are alike, they are alike only in number, extension, motion and duration; they are unlike in association, position, direction or motion and the duration of association, so that likeness and unlikeness is inherent in matter itself. In bodies innumerable combinations of number, extension, motion and duration are found, and out of these are developed innumerable likenesses and unlikenesses, so that one body is like another in many respects and unlike that other in many other respects. The science of classification takes these likenesses and unlikenesses and discovers degrees among them which are of profound importance in the study of the world, and upon which a large share of knowledge rests. All knowledge does not rest upon likeness and unlikeness; but like-

ness is founded upon number, and men have discovered that what is true of a body is true of any other body of like kind, under the axiom that whatever is true of anything is true of its identity in so far only as it is a constant property or an absolute, and not in so far as it is a variable or relative. These are all simple, self-evident propositions, but in the compounding and recompounding of matter it is not always possible to disentangle the constants from the variables. Men lost in the meaning of words, forever wandering in linguistic jungles, have engaged in discussions about essences and have at last reified the word as something which is not number associated with extension, motion and duration, but as some occult existence unknown and unknowable, which gives to bodies their likeness or unlikeness. Having reached the conclusion that matter is something more than its constituents, with an occult, unknown and unknowable substrate, they take the next step that the essence of class or likeness and unlikeness exists not in the fundamental properties of body or the fundamental constituents of matter, but in their substrate.

All known things are classified either properly or improperly. The characters upon which they are classed are thus innumerable. These characters which constitute class are all the bodies embraced in the class and all the properties embraced in all the bodies of the class. The term essence, then, used in this sense, means all of these things. Therefore it is a general name for everything in the universe, but obtaining its particular meaning in any case by the context. What is the meaning of the word *this?* It may be applied to any constituent of matter, to matter itself, to any body or to any property, relation or quality in the material world, and to any idea in the mental world, and its meaning is derived from the context; it has no definite meaning in itself. *Essence*, as a word used by philosophers, is a pronoun of like character without specific meaning, and attains its specific meaning only by the context; it has one meaning at one time, and another at another, and thus it seems to be illusive. As the substrate of matter, a reified nothing, is entertained in the minds of some as an entity, so some thinkers make essence a property of this substrate—a nonenity of a nonenity. Chuar, Hegel and Spencer reason in this manner. Essence as connoting the essential characters of a class is a word the meaning of which scientific men clearly understand; it is never ambiguous, although naturalists may sometimes disagree about the essentiality of a particular character, but the essence of which the philosopher thinks is nonexistent, the opinions of the three wise men to the contrary notwithstanding.

SPACE.

The word space is the pronoun of all extensions, figures and structures of extensions in the multitudinous bodies of the world. There are many extensions, and every known body is a constituent of some other body, and this synthesis may be continued until the mind is lost in immensity. The space occupied by a body is its extension in structure and figure. This desk before me has extension, or we say that it occupies space; the space which it occupies is its extension, from which it excludes other bodies. Remove the furniture from the room, it is said to be empty, yet it is full of air; remove the air from the room, yet it is full of ether; remove the ether, may be, we know not, all is removed; then the wall encloses void—nothing—but the walls of the room yet have extension, and we can measure this by measuring the walls, but void cannot be measured; there is nothing to be measured. Thus it is that space is the pronoun of all dimensions of all bodies, severally and conjointly, and as they are variable, space seems to be illusive, and it comes

at last in the minds of careless thinkers to mean something more than extension, an unknown and unknowable thing that, like essence, belongs to the unknown and unknowable substrate of matter. The word is useful when its use is understood as a pronoun or general word whose meaning is given by the context.

FORCE.

Force is the pronoun for combinations of motion. It thus may be applied to numerous things now existing, or which have existed in the past or may exist in the future. It is the general word for all collisions and all combinations of collisions; collisions of particles of ether in light and heat, collisions of particles of air in sound, collisions of particles of water in stress, collisions of particles of matter in all solids exhibited in the structure and strength of those materials. It thus stands for the action of two or more bodies as they come in collision, and thus influence each other's motions. It is not an occult, unknown or unknowable something which belongs to an occult, unknown and unknowable substrate. The term has no particular or determined meaning in itself, but derives its meaning from the context. It is a word of universal use, whose meaning must be determined by its application; it is the general term or pronoun to denote any or all actions and reactions.

TIME.

Time is the pronoun of all durations. It means any duration to which the term is applied, all durations or any collocation of durations the mind may entertain. When reified it comes to be thought of as applying to an existence independent of the things which have duration. Then time, like essence, space and force, becomes a property of the substrate of matter, an illusion about an illusion.

GHOST.

Spirit is a general term or pronoun for all judgments in the infinite variety of sensations, perceptions, understandings, acceptions and reflections. It is a name for all ideation. It is known to us only in its association or connection with the universal constituents of matter, which are number, extension, motion and duration. There is no spirit which is not a unity of many and one. There is no spirit which has not force. There is no spirit which has not duration ; in so far all are agreed; and it is here affirmed that there is no spirit which has not extension, for without extension all the other constituents would vanish, become nothing, absolutely unimaginable or unthinkable. When spirit is considered to be something which is not number or many in one, which has not extension with figure and structure without force, or the power of action and reaction and without duration as persistence or persistence and change, that is, without time, it becomes a nonentity, a nothing, and it is then an illusion and is usually called ghost.

CAUSE.

We use the word cause as we use the words *this* and *that*, as a general term or pronoun for anything that stands in relation to any other thing in the production of a change. The multitudinous bodies and particles of the universe coöperate with one another in the production of changes. The condition before a particular change is considered in respect to the condition after the change, and the condition which coöperated in the production of the change, is called a cause, and the condition after the change is called an effect. It is thus that the term cause may be applied to any body, to any property, or to any relation; it is a term for any of these things, any collocation of these things or any part of these things, and just what its meaning may be can be discovered

only by the context in which the word is used. In the multitude of bodies, properties and relations which coöperate in the production of the change whose result is called an effect, we may stop to consider any one and call that the cause. Failing to appreciate the variable significance of the word, men are led into the illusion that there is some entity, some separate existence called cause.

Metaphorically, essence is sometimes used for space, sometimes for force, sometimes for time, sometimes for spirit, and sometimes for cause, and interchangeably all of these terms may be used as metaphors for one another.

Thus it is that we have a family of chimeras in substrate, essence, space, force, time, ghost and cause that are not bodies or the properties of bodies, but things nonexistent—mysteries that are at the foundation of all philosophies of the unknowable and all philosophies of the contradictory, and the ground of all antinomies. They constitute the substrate, the essence, the space, the time, the cause of the philosophies of the three wise men, Chuar, Hegel and Spencer.

We shall hereafter see more clearly how these illusions have been developed and how other illusions have gathered about them. Here we simply call attention to the fundamental illusions to indicate somewhat the purposes of this argument.

It is within the experience of every human being, and has been through all generations, that man is forever discovering number, extension, motion, duration and judgment. He learns something of number in infancy and adds to his knowledge daily and extends his knowledge to an indefinite multiplicity. He adds to his knowledge the extension of one body and another still embodied in a higher order; and thus his knowledge of extension increases to an indefinite extent. He is forever discovering new motions and new combinations of motions as forces and finds that he is able thus to add more and more of like motions and forces to his knowledge. Ever he is discovering durations - the durations of coexistent things and the durations of past things, extending to high antiquity, and he prophesies durations to come, and many do come, until his mind is led into the illimitable future. Mind is then trained by constant experience to expect a further enlargement of knowledge and to consider the possibilities into which it may expand, until it dwells upon endless number, endless extension, endless force, endless duration. Man contemplates multiples and submultiples of the things of which he already has knowledge, and then invents implements of research by which submultiples are discovered, and other implements by which multiples in higher orders are discovered. Finding that he has explored but a small part of the universe, and that within the universe wherever bodies are to be met they have been resolved into numbers, extensions, motions and durations, he grasps the idea of infinity not as something other than that of which he knows, but as more of that which he best knows. The experience of men through countless generations has organized the concepts of number, extension, motion and duration as the universal factors of matter, and never has any mind discovered any other things saving only those which are included in the terms of mind. Of matter without mind, man has absolutely no vestige of knowledge which is not included under the terms number, extension, motion and duration. These terms absorb them all. Therefore matter is number, extension, motion and duration, and at least some matter has judgment.

The mind discovers another factor or category in the universe—judgment, which develops into cognition of the constituents of matter, of their relations, and also a cogni-

tion of cognitions and the relations of cognitions. It is thus that the universe is resolved into material elements and judgments, the five things best known, and science in dealing with the universe explains them by resolving them into these best known things. Science does not lead to mystery, but to knowledge, and the mind rests satisfied with the knowledge thus gained when the analysis is complete—when any newly discovered body is resolved into its constituents or any new idea into its judgments.

Concepts of number, extension, motion, duration and judgment are developed by all minds; from that of the lowest animal to that of the highest human genius. Through the evolution of animal life, these concepts have been growing as they have been inherited down the stream of time in the flood of generations. It is thus that an experience has been developed, combined with the experience of all the generations of life for all the time of life, so that it is impossible to expunge from human mind these five concepts. They can never be cancelled while sanity remains. Things having something more than number, extension, motion, duration and judgment cannot even be invented; it is not possible for the human mind to conceive anything else, but semblances of such ideas may be produced by mummification of language.

Ideas are expressed in words which are symbols, and the word may be divested of all meaning in terms of number, extension, motion, duration and judgment and still remain, and it may be claimed that it still means something unknown and unknowable; this is the origin of reification. There are many things unknown at one stage of experience which are known at another, so man comes to believe in the unknown by constant daily experience; but has by further converse with the universe known things previously unknown, and they invariably become known in terms of number, extension, motion, duration and judgment, and are found to be only combinations of these things. It is thus that something unknown may be imagined, but something unknowable cannot be imagined.

No man imagines reified substrate, reified essence, reified space, reified force, reified time, reified ghost, or reified cause. Words are blank checks on the bank of thought, to be filled with meaning by the past and future earnings of the intellect. But these words are coin signs of the unknowable and no one can acquire the currency for which they call.

Things little known are named and man speculates about these little known things and erroneously imputes properties or attributes to them until he comes to think of their possessing such unknown and mistaken attributes. At last he discovers the facts; then all that he discovers is expressed in the terms of number, extension motion, duration and cognition. Still the word for the little known thing may remain to express something unknown and mystical, and by simple and easily understood processes he reifies what is not, and reasons in terms which have no meaning as used by him. Terms thus used without meaning are terms of reification.

Such terms and such methods of reasoning become very dear to those immersed in thaumaturgy and who love the wonderful and cling to the mysterious, and, in the revelry developed by the hashish of mystery, the pure water of truth is insipid. The dream of intellectual intoxication seems more real and more worthy of the human mind than the simple truths discovered by science. There is a fascination in mystery and there has ever been a school of intellects delighting to revel therein, and yet, in the grand aggregate, there is a spirit of sanity extant among mankind which loves the true and simple.

Often the eloquence of the dreamer has even subverted the sanity of science, and clear-headed, simple-minded scientific men have been willing to affirm that science deals with trivialities, and that only metaphysics deals with the profound and significant things of the universe. In a late great text-book on physics, which is a science of simple certitudes, it is affirmed:

To us the question, *What is matter?*—What is, assuming it to have a real existence outside ourselves, the essential basis of the phenomena with which we may as physicists make ourselves acquainted?—appears absolutely insoluble. Even if we become perfectly and certainly acquainted with the intimate structure of what we call Matter, we would but have made a further step in the study of its properties; and as physicists we are forced to say that while somewhat has been learned as to the properties of Matter, its essential nature is quite unknown to us.

As though its properties did not constitute its essential nature.

So, under the spell of metaphysics, the physicist turns from his spectroscope to exclaim that all his researches may be dealing with phantasms.

Science deals with realities. These are bodies with their properties. All the facts embraced in this vast field of research are expressed in terms of number, extension, motion, duration and judgment; no other terms are needed and no other terms are coined, but by a process well known in philology as a disease of language, sometimes these terms lapse into meanings which connote illusions. The human intellect is of such a nature that it has notions or ideas which may be certitudes or illusions. All the processes of reasoning, including sensation and perception, proceed by inference; the inference may be correct or erroneous, and certitudes are reached by verifying opinions. This is the sole and only process of gaining certitudes. The certitudes are truths which properly represent noumena, the illusions are errors which misrepresent noumena. All knowledge is the knowledge of noumena, and all illusion is erroneous opinion about noumena. The human mind knows nothing but realities and deals with nothing but realities, but in this dealing with the realities—the noumena of the universe—it reaches some conclusions that are correct and others that are incorrect. The correct conclusions are certitudes about realities; the incorrect conclusions are illusions about realities. Science is the name which mankind has agreed to call this knowledge of realities, and error is the name which mankind has agreed to give to all illusions. Thus it is that certitudes are directly founded upon realities; and illusions as they are always about realities, are thus indirectly, though incorrectly, founded upon realities, but certitudes and illusions alike all refer to realities. In this sense then it may be stated that all error as well as knowledge testifies to reality, and that all our knowledge is certitude based upon reality, and that illusions would not be possible were there not realities about which inferences are made.

Known realities are those about which mankind has knowledge; unknown things are those things about which man has not yet attained knowledge. Scientific research is the endeavor to increase knowledge, and its methods are observation, experience and verification. Illusions are erroneous inferences in relation to known things. All certitudes are described in terms of number, extension, motion, duration and judgment; nothing else has yet been discovered and nothing else can be discovered with the faculties with which man is possessed.

In the material world we have no knowledge of something which is not a unity of itself or a unity of a plurality; of something which is not an extension of figure or an

extension of figure and structure; of something which has not motion or a combination of motions as force; of something which has not duration as persistence or duration with persistence and change.

In the mental world we have no knowledge of something which is not a judgment of consciousness and inference; of a judgment which is not a judgment of a body with number, extension, motion and duration. Every notion of something in the material world devoid of one or more of the constituents of matter is an illusion; every notion of something in the spiritual world devoid of the factors of matter and judgment is an illusion. These are the propositions to be explained and demonstrated.

In the following chapters an attempt will be made to show that we know much about matter, and although we do not know all, all we know is about matter in its categories of number, extension, motion, duration and judgment, or that we know of matter in its four categories and that we know of mind in the categories of judgment, but always this mind is associated with matter. In doing this we shall endeavor to discriminate between the certitudes and illusions current in human opinion.

In the intoxication of illusion facts seem cold and colorless, and the wrapt dreamer imagines that he dwells in a realm above science—in a world which as he thinks absorbs truth as the ocean the shower, and transforms it into a flood of philosophy. Feverish dreams are supposed to be glimpses of the unknown and unknowable, and the highest and dearest aspiration is to be absorbed in this sea of speculation. Nothing is worthy of contemplation but the mysterious. Yet the simple and the true remain. The history of science is the history of the discovery of the simple and the true; in its progress illusions are dispelled and certitudes remain.

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NOTES ON THE DENSITY AND TEMPERATURE OF THE WATERS OF THE GULF OF MEXICO AND GULF STREAM.*

IT is estimated that the evaporation in the Gulf of Mexico amounts to about 60 inches a year, thus diminishing the amount of water in the Gulf 1.54 cubic miles per day. The evaporation is greatest in the central parts of the Gulf, following a line from east to west and approximately coinciding with the line of mean maximum of atmospheric pressure.

Precipitation, on the other hand, is greatest in the southwestern and northeastern parts of the Gulf, and least in the area intervening between the sandy plains of Yucatan and the arid regions of southern Texas and northern Mexico. By computation we find it to reach 32.7 inches annually, which is about 55 per centum of the evaporation, and it increases the waters of the Gulf by 0.84 cubic miles per day.

The water supply is further increased by river discharges, which amount to about 0.68 cubic miles per day; nearly 70 per centum of this volume being furnished by the Mississippi River. It will be seen that precipitation and river discharges feed the gulf by nearly the same amounts, but the effect produced by those feeders sinks into insignificance when compared with that produced by the inflowing current of the Yucatan Channel, which, according to a calculation from Lieut. Pillsbury's current observations, hurls the enormous quantity of 652 cubic miles of water per day into the Gulf, which quantity by itself would suffice to raise the level of the entire Gulf $5\frac{3}{4}$ feet within that space of time.

The Gulf stream carries off only about two-thirds of the water that is added to the volume of the Gulf in the manner indicated above, and evaporation being power-

* Abstract of a paper read to the Philosophical Society of Washington, by permission of the Superintendent of the U. S. Coast and Geodetic Survey.

less to remove the other third we are led to the conclusion that it flows back into the Caribbean as an undercurrent.

A study of the proportions of these currents has led to the conclusion that the Yucatan Channel current owes its existence mainly to the mechanical effect of the winds which produce an accumulation of waters in the northwestern part of the Caribbean Sea, but it is also, in part, due to differences in temperature and density between the waters of the latter and those of the Gulf. Hence, should the winds cease to influence the level of the Caribbean Sea, there would still be a surface current from this sea into the Gulf and an undercurrent in the opposite direction, similar to those which actually exist in the Strait of Gibraltar from and to the Atlantic Ocean.

The fresh water, which finds its way into the southwestern part of the Gulf, remains on the surface of the Gulf waters, but on account of its high temperature it readily assimilates with the sea water, and by continuous absorption of salt and heat from the lower strata reduces the latter to abnormally low temperatures.

The river and rainwater entering the northern parts of the Gulf also remains on the surface, but it preserves its distinctive character and low specific gravity for a much longer time period, owing to its comparatively low temperature, for not until it has reached the middle of the Gulf has it gathered salt and heat to its full capacity. Thus the course of the waters of the Mississippi River can be traced by their lightness for hundreds of miles into the Gulf of Mexico. Instead of flowing directly southeast towards the Strait of Florida, in accordance with the generally accepted supposition, these waters flow to the westward, which deflection undoubtedly is influenced by the existence of a lower water level in the western part of the Gulf, due to the piling up of the water in the eastern part by the flow from the Yucatan Channel. Notwithstanding the fact that the tendency of the Mississippi River waters, after entering the Gulf, is towards the west, and regardless of the strength of the inflowing Yucatan current, the predominent surface drift of the Gulf is towards the Strait of Florida, which phenomenon may be explained by assuming that the Yucatan current in its west and northward progress dips below the surface waters and continues as an undercurrent.

The surface waters of the central and eastern parts of the Gulf of Mexico, being propelled against the direction of the prevailing winds, are subjected to a powerful influence of evaporation, by which their specific gravity is increased to such an extent that their weight can no longer be borne on the surface, and sinking, they carry larger amounts of salt and heat into the deep strata than could reach such great depths in any other way. Thus only can we account for such temperatures as 60° and more at a depth of 250 fathoms, occurring off Cape San Antonio, half way between the Florida and Campeche Banks, against 44° in the western part of the Gulf and 47° in the Caribbean Sea at corresponding depths.

In conformity with the direct effects, known to result from decided differences of temperature at considerable depths in communicating parts of the ocean, there will be an undercurrent from the southeastern part of the Gulf toward the western part and another entering the Caribbean Sea, supporting the views expressed when considering the volume of water.

It is a remarkable phenomenon that the temperature in the substrata of those parts of the ocean adjacent to the Strait of Florida should be so nearly the same as that of the eastern part of the Gulf, thus precluding the existence of a subsurface counter-current in that strait; and a singu-

lar coincidence may be noted in the general character of the bed of that strait, it being only sufficiently deep to permit the passage of the Gulf Stream. It must not be supposed, however, that the under-current flowing into the Caribbean Sea entails a permanent saline and thermal loss upon the waters of the Gulf, as those abducted quantities of salt and heat, by a system of transfers, find their way into consecutively higher levels, and finally reach the surface current and return with it to the Gulf.

The current of the Yucatan Channel, notwithstanding its being the strongest current of the entire Gulf Stream system, possesses no great depth, and owing to its rapid spreading out it soon loses the best part of its velocity. The only exception in this respect is met with along the northern edge of Campeche Bank, where its flow shows considerable vitality, and it is here that it has evidently taken the shortest route to reach the western part of the Gulf.

It also appears to be very variable in its strength; when flowing at its best some of its waters are sent into the Strait of Florida, but its main strength is directed against the Gulf of Mexico with the effect of penning up its waters above the level of the Atlantic. Whenever the Yucatan current relaxes in activity, the waters of the Gulf of Mexico, in their reaction, frequently succeed in cutting it off altogether from reaching the Strait of Florida, and sometimes even in partly forcing it back at its eastern and weakest flank, into the Caribbean Sea.

The Gulf Stream, as has been shown by Lieutenant Pillsbury, is not the direct continuation of the Yucatan Channel current, but originates about in the middle of the western entrance to the Strait of Florida.

As it first appears in the Strait it is comparatively an insignificant current, and we are also disappointed in not finding it that fiery furnace which, according to its reputation, transmits sufficient heat to the eastern part of the Atlantic to modify the climate of the whole of western Europe. The fact appears to be that it does not start upon its journey at this point with more heat than it requires for its own use until it reaches Cape Florida, as it at once enters a contest against the cold waters descending from the Florida Bank, extending nearly halfway across the Strait. During its progress through the Strait these cold waters are forced back into the vicinity of the reefs, and by the time the Gulf Stream has reached Cape Florida it is in full possession of the Strait, from the surface to the bottom, and from the Bahama Banks to the Florida Reefs; its axis being but 15 miles distant from Cape Florida.

This victory, however, has been obtained at a great sacrifice of its supply of salt and heat, leaving it in an inadequate condition to engage unaided in another contest which it must immediately enter upon. Fortunately reinforcements are at hand, warm and highly saline waters, which have been slowly advancing along the Bahama Bank, join the Gulf stream at its point of weakness. Other and far more important succor gathered by the northeast trade winds, joins the Gulf stream on entering the ocean at the eastern end of the strait. Yet all these additions cannot account for the observed fact that the waters of the Gulf stream are so much warmer and more saline than those of the ocean, and in order to discover the source of this great heat we must look in a different direction than towards the Gulf of Mexico, or towards the surface drift of the Atlantic.

What has been described as taking place on the surface of the southeastern part of the Gulf is reënacted on a much larger scale on the entire surface of that part of the Atlantic Ocean lying between the Bermudas and the 'continental shelf,' off the Southern States. A powerful evaporation caused by the trade winds produces a con-

densation of the warm surface water, which sinks into greater depths and imparts a higher degree of temperature and salinity to the substrata than are met with in any other ocean. The waters of these substrata having a temperature from 60 to 64 degrees, at a depth of 250 fathoms, meet the cold waters, in a space about 40 miles wide, descending along the edge of the continental slope, which at the same depth (250 fathoms) have a temperature of only about 45 degrees.

Within this space of forty miles' width a transition of heat and salt is effected, resulting in an entire reconstruction of the superincumbent stratum of water, producing that peculiar distribution of salt and heat at the surface that is characteristic of the Gulf stream. When warm seawater comes into contact with colder seawater it becomes heavier, for the reason that the increase of density, due to loss of heat, surpasses the decrease, due to the loss of salt. When this occurs in the depths of the ocean the warm water will sink to still greater depths, but here (as also on the slopes of great submarine banks like the Bahama, Florida and Campeche Banks) this dense and warm water touches bottom, and another shift must be made to dispose of the excess of salt, the maintenance of equilibrium being a physical necessity.

The density of warm water is less affected by the addition of a certain quantity of salt than cold water would be, and for this reason the excess of salt and heat at the bottom, on the inner edge of the Gulf Stream, shifts to higher levels where, in consequence of higher temperatures, larger quantities of salt can be stowed away with less change of density than at greater depths. Thus, by a withdrawal of salt and heat from the greater depths and their accumulation at the surface, that peculiar distribution is attained which characterizes all the serial temperature observations of the Gulf Stream sections, including those obtained by the Challenger.

Observations show the highest specific gravities of the Gulf Stream waters to be in the latitudes of Capes Lookout and Hatteras, exceeding those of all other parts of the open ocean, and surpassed only by those of the Red Sea and of the western part of the Mediterranean.

Although the 'upheaval' of the waters of the Gulf Stream develops first in upward currents, in the substratum in which the transition of heat and salt begins, it is not improbable that these currents, like the winds in aërial circulation, may assume a more or less horizontal direction in their progress to the surface. It may also be assumed that the storage of heat in the surface stratum is not without influence upon the level of the Gulf Stream, and that this difference of level between the Gulf Stream and the adjacent areas of the ocean may call other currents into life, but a farther consideration of these subjects would lead us into the sphere of the so-called dynamics of the Gulf Stream, a field already ably discussed and sufficiently studied.

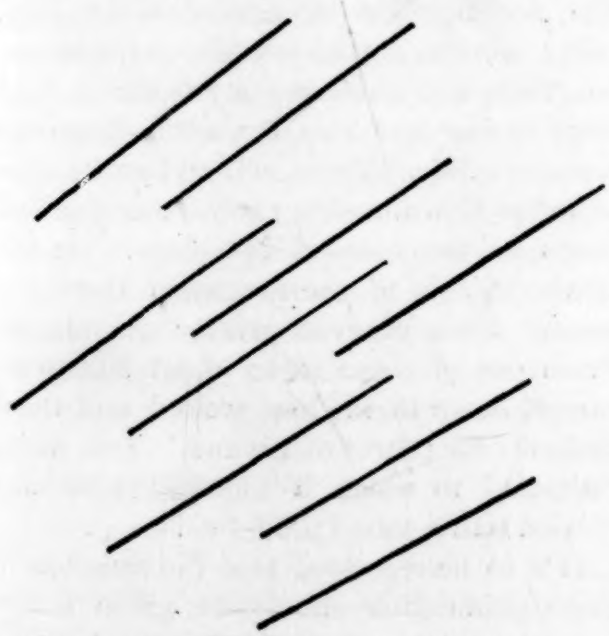
A. LINDENKOHL.

AN OPTICAL ILLUSION.

THE brilliant electric lights on the borders of the lake in the Baltimore park have served to call my attention to a phenomenon which is so very familiar that one is wholly disinclined to regard it as a 'phenomenon' at all. I refer to the fact that the long stream of light reflected by the surface of the water from a lamp on the opposite side does not look like an object lying upon the surface, but like a bright post projecting down into the water, in continuation of the lamp-post. This is without doubt a particular case of the illusion by which lines which have any position whatever in planes passing through the axis of the body (or, for small near objects, in planes passing through the vertical meridian

of the one eye with which they are looked at)* are taken by us to be vertical lines. This illusion is illustrated in Fig. 1. The lines of the figure are all drawn through a common point about three inches beyond the corner of the paper. If one eye be put in the position of this point (the other being closed), and if the paper be held horizontally about on the level of the eye, the lines will all seem to stand upright. The reason is that when one eye only is used, we have very small ground for knowing how such a line is situated in the plane determined by it and by the nodal point of the eye, and hence we take it to be a vertical line *faute de mieux*, because by far the greatest number of lines which strike the retina in this meridian are vertical lines. With many lines, the illusion is stronger than with one, because every group of vertical staves that we have ever seen has looked like this, and it has probably never happened to us to see a group of lines lying on the ground in just this position.

That this is the correct explanation of the phenomenon of the lights is confirmed by the fact that, upon looking at the reflection with the head inclined through an angle of ninty degrees, the illusion wholly disappears. One can no longer believe that it is possible to see the stream of light otherwise than as lying flat upon the surface of the lake. In this case the image of the line of light falls along the eyes, from one to the other, or just as a line would do which went from right to left if the head were in its normal position. Such a line we have no tendency to see vertical, and hence we now see the streak of light where it really is on the surface of the water. With the head wholly inverted, the line becomes vertical again, but less strongly so than when the head is in the customary attitude.



*Am. Jour. of Psychology, I., 101 and James' *Principles of Psychology*, II., 95.

CHR. LADD FRANKLIN.

CURRENT NOTES ON PHYSIOGRAPHY.

THE TERTIARY PENEPLAIN IN MISSOURI.

THE prevalent opinion that the 'mountains' of the dissected Ozark plateau in Missouri are old geographical features meets welcome contradiction in an essay by Keyes, State Geologist (Missouri Geol. Survey, viii., 1894, 317–352). The relatively even upland surface of the plateau is explained as a peneplain of denudation; and the dome-like form of the plateau today is regarded as the result of elevation since the close of the Tertiary. The general upland plain is dissected by steep-sided or canyon-like trenches, in which the process of deepening is still continued. "The last elevation is not yet ended, and the changes of level in the region are probably going on now as rapidly as they ever have in the past geological time" (p. 352). While the strata are nearly horizontal in the Ozark plateau, they are tilted in the Ouachita mountains, south of the broad valley of the Arkansas river, in the State of that name. Keyes regards the relatively even crest lines of the Ouachita ridges as representing the same peneplain as that of the Ozarks; the broad valley of the Ar-

kansas being a trough of erosion between the two highland areas, 'due partly to structural peculiarities, but it is also due largely to other conditions,' the latter not being specified. "As a unit, the Tertiary peneplain was bowed up from the Red river to the Missouri."

It has for some time been desirable to fix the date of the Ozark peneplain, but unfortunately the evidence by which a Tertiary date is here assigned to the completion of the peneplain and a Post-Tertiary date to its uplift and dissection is not fully stated. The narrowness of the valleys may, however, certainly be taken to 'emphasize the fact that the Ozark uplift of to-day is essentially modern.'

HIGH LEVEL GRAVELS OF KENTUCKY.

THE rolling limestone uplands of the blue-grass region of Kentucky, rimmed around by sandstone escarpments on the south, and dissected by deep narrow valleys of streams that flow to the Ohio on the north, are strewn over at various places with gravels and sands. The distribution of the gravels is discussed by A. M. Miller, of Lexington, Ky. (Amer. Geol., XVI., 1895, 281–287). These loose materials are water-worn and bedded, and are derived mostly from the harder rocks of the enclosing escarpments; they are found chiefly near existing valleys. Miller concludes that within comparatively recent times the rivers were flooded to a height of 300 to 350 feet above their present channels. In explanation of such flooding, a glacial obstruction of the Ohio is considered as a possibility, but satisfactory evidence is not found in favor of it. 'Submergence' of unspecified nature is also mentioned without reaching any definite conclusion about it.

No consideration is given to the possibility that the gravels may have been spread over the upland surface before the present canyon-like valleys were eroded, while the whole region stood at a lower level than at present, but not submerged. This is eminently possible, for the aspect of the blue-grass region is strongly suggestive of base-levelling during a former lower stand of the land, and of dissection after elevation to the present altitude, as has been suggested by Westgate (Amer. Geol., XI., 1893, 258–259). The prepossession that the upland gravels could not endure for so long a time as would be needed to carve the canyon-like valleys is not well supported. Old river gravels lie on rock benches enclosing the gorge of the upper Ohio; and in similar position on the valley slopes of the Meuse in its transverse path across the Ardennes; even the fine loess of the upper bench of the Rhine valley in the Schiefergebirge is older than the narrow gorge of that energetic river.

CLOUD-BURST TRACKS AND WATER GAPS IN ALABAMA.

A REPORT on the Coosa coal field by A. M. Gibson (Alabama Geol. Survey, 1895) gives a description of two great scars on Coosa mountain, produced by cloud-bursts that accompanied the tornadoes of July, 1872. On the northwest side of the mountain there is a washout sixty feet wide and three or four feet deep, extending down the mountain side. Trees, soil and rocks were all swept down, making great moraine-like heaps at the base of the slope. On the southeast side of the mountain there are several scars of even greater magnitude. From one of these rocks of all sizes were carried down to the low ground and there heaped over 'acres of ground.' One mass, estimated to weigh a hundred tons was carried half a mile (p. 28–30).

It is to be regretted that the sanction of State publication should be given a few pages later to an antiquated account of 'Big Narrows' in Double mountain. "Some convulsion of nature must surely have made the

break that let the waters enter here, it else seems impossible that this stream could have cut through such rocky masses by a gorge so narrow, and leaving so little sign of abrasion on the perpendicular cliffs" (p. 32). If there were really reason to regard this gap as the result of a convulsion of nature it would deserve to be carefully described; and such a rarity would become a mecca for geologists and geographers; but as there appears to be no sufficient ground for thinking it different in origin from the hundred other water gaps of the Appalachians, the people of Alabama ought to have a reasonable explanation of its method of production.

MASSANUTTEN MOUNTAIN, VIRGINIA.

A PRELIMINARY account of this peculiar sandstone mountain, rising from the limestone floor of Shenandoah Valley, is given by A. C. Spencer (Johns Hopkins Univ. Circ., No. 121, Oct., 1895, 13, 14). The mountain is of complicated synclinal structure, the resistent sandstone which forms its rim being bent into the form of a long, narrow, deep and wrinkled trough, whose bottom dips 1,000 feet or more beneath the surrounding valley floor. The greater part of the crest line of the mountain represents the much dissected Cretaceous peneplain of the Appalachian province; but certain points rise to greater elevations by as much as 500 or more feet. Passage creek, draining the northern portion of the valley enclosed by the mountain rim, is peculiar in cutting its outlet gap at the apex of the syncline, instead of to one side, as is commonly the case in Pennsylvania.

W. M. DAVIS.

HARVARD UNIVERSITY.

CURRENT NOTES ON ANTHROPOLOGY.

ETHNOGRAPHIC SURVEYS.

IT has been already mentioned in these notes (see SCIENCE, Feb. 10, 1893,) that an ethnographic survey of Great Britain and Ireland had been instituted under the auspices of the British Association for the Advancement of Science. Already two preliminary reports have been made, and quite lately the Honorable Secretary of the Committee, Mr. E. Sidney Hartland, has published some explanatory notes about the plan, in the 'Transactions of the British and Gloucestershire Archæological Society.' These are very useful and suggestive, and together with the forms of schedule prepared by the Committee should be secured by students of ethnography as showing the well-matured methods of investigation decided upon by the high authorities in charge of the survey. They may be had by addressing 'the Secretary of the Ethnographic Survey, British Association, Burlington House, London, W.'

THE EARLY USE OF METALS IN EUROPE.

DR. JULIUS NAUE, the well known editor of the *Prähistorische Blätter* in Munich, contributes to the 'Revue Archæologique' an instructive article on the Hallstatt Epoch in Bavaria and the Palatinate, principally from his own researches.

His epoch is that of 'the first age of iron' and begins about 800 B. C. At its beginning bronze was much more abundant than iron, and the forms given it were graceful. The bodies were generally incinerated and placed in stone tombs. Long, leaf-shaped swords of iron were laid with the warriors, and ornamented vases of pottery beside them. Knives, daggers, pins, lance points and ornaments of both metals are common. The ethnographic conclusion is that these were Celtic tribes, probably the Licatii, of Latin authors. In agriculture they were skilled and in commerce had established distant relations.

Their contemporaries in the Upper Palatinate were less advanced, being addicted to human sacrifices and more warlike.

ANTHROPOMETRY OF THE AMERICAN INDIANS.

At a session of the Berlin Anthropological Society, in May last, Dr. Franz Boas reported the results of numerous measurements of American Indians and half-breeds, which he had carried out. A few of his conclusions may be mentioned.

On the whole, the Indian is rather tall, and the half-breeds slightly taller than the pure blood. The women are 92 to 94 per cent. the height of the male. As usual, the tallest tribes are dwellers in plains. The head-form varies extremely, but is persistent over wide regions, the Mississippi valley being peopled with mesocephalic tribes, the extreme north with dolichocephalic, while others, as the Téné, both north and south, are brachycephalic. There is no general type of native American skull. The facial diameter rarely sinks below 147 mm., and when such is the case foreign blood may be suspected.

The article is furnished with abundant tables and diagrams, and offers a fine example of scientific work.

THE MONUMENTS OF YUCATAN.

The first number of the anthropological series published by the Field Columbian Museum, Chicago, is the 'Archæological Studies among the Ancient Cities of Mexico,' by the curator, William H. Holmes. The first part, which alone has appeared, is devoted to the architectural remains of Yucatan. These were explored by the author in a visit there last winter, which included an inspection of the relics at Mugeres Island, Cozumel, Uxmal, Izamal, Chichen Itza, and some places of less note.

The results fill a volume of 137 pages, abundantly illustrated and rich with accurate observations and careful deductions. Several sketch maps and panoramas of the sites are inserted which give a much clearer notion than can be obtained from verbal descriptions. The analysis of the elements of Mayan architecture are especially original and valuable and impart a peculiar worth to this monograph. The same may be said of the observations on the materials employed, the orientation, the necessity for instruments of precision, the function of the buildings, the dressing of stone, the evolution of the ground plans, stairways and substructures, etc. In fact, the reader will find on almost every page something to catch his attention and to cast new light on the many obscure problems connected with the ancient Mayas.

D. G. Brinton,

SCIENTIFIC NOTES AND NEWS.

A PERMANENT SCIENTIFIC HEAD FOR THE U. S. DEPARTMENT OF AGRICULTURE.

An amendment to the Agricultural appropriation bill has just been sent to Congress providing for a "Director-in-Chief of scientific bureaus and investigations, to serve during good behavior, to have authority to act as Assistant Secretary, and to perform such other duties as the Secretary may direct."

This amendment, which has received the endorsement of the Secretary and Assistant Secretary of Agriculture, is the outgrowth of an effort to secure a permanent non-political organization and administration of the various bureaus and divisions engaged in the scientific work of the Government, and at the same time bring about a more intelligent and more effective coöperation than has been heretofore possible.

The chief promoters of this movement are well-known public-spirited educators and men of science entirely outside of the Government service.

The Department of Agriculture as at present organized comprises a large number of scientific and administrative divisions having for their object the discovery, exploration and development of the agricultural and other natural resources of the country. The scientific divisions are engaged in researches requiring the highest technical skill, and some of them in the solutions of problems requiring long years of preparation and scientific training.

Excluding the Weather Bureau, no less than eight divisions are doing work which in the main is purely scientific, and each of these has its independent laboratory or laboratories. Including the Weather Bureau and the meat inspection service of the Bureau of Animal Industry, 993 of a total of 2,019 employees are engaged chiefly upon scientific and technical subjects, and $1,700,000 of the $2,400,000 appropriated for the Department of Agriculture is expended upon this work. But the greater part of the work of the Weather Bureau and Bureau of Animal Industry, while fundamentally scientific in method and character, is not in the line of original investigation, and therefore may be omitted in the present statement. Still, each of these Bureaus conduct at Washington certain investigations in pure science, the cost of which, added to that of the eight scientific divisions already mentioned, amounts annually to nearly half a million dollars. Nevertheless no coöperative organization or classification of these scientific divisions, except those of the Weather Bureau, has been as yet undertaken.

It would seem a simple business proposition, needing no argument, that this comprehensive and vastly important work, promoting, as it does, the development of almost every resource of our land and every industry of our people, and concerning the food and health of a large part of our population, should have a permanent, broadly educated and experienced scientific head, free from the disquieting influence of politics.

The first, and in some respects the most difficult, step toward the accomplishment of this end was taken when Secretary Morton secured for the Department of Agriculture the protection of the Civil Service, thus putting an end to the terrors of political pressure in filling vacancies in the scientific divisions.

Should the amendment now before Congress become a law—and it is believed the friends of science and education throughout the land will give it their unqualified support—it is by no means improbable that other scientific bureaus of the Government will seek the protection and support provided thereby, and that in the near future we may boast a National Department of Agriculture and Science.

ASTRONOMY.

THE Lick Observatory has just published 'Contributions,' No. 5, a volume of 86 pages octavo, devoted to meteor and sunset phenomena. One of the most interesting papers in the volume is by Prof. Schaeberle, and contains a discussion of a series of meteor observations made simultaneously at Mount Hamilton and at Mount Diablo, forty miles distant. The Mount Hamilton observations were made by Messrs. Colton and Perrine; those at Mount Diablo by Mr. Schaeberle. The formulæ needed for the complete reduction of observations of this kind, including the criterion for determining whether the observations of both stations in any given instance really refer to the same meteor, are fully developed. Nine meteor paths were successfully worked out in this way in August, 1894. The heights of the meteors range from four to fifty-seven miles. Prof. Schaeberle concludes by pointing out that much more reliable methods of observation must be devised, if orbits having any approach to precision are to be secured for meteors. We can only hope that the experiments now in progress at the Yale College Observatory will lead to the possibility of observing these interesting bodies photographically.

WE learn from the last number of the Publications of the Astronomical Society of the Pacific that several important instruments have recently been completed at the works of Mr. Saegmuller in Washington. These include a nine-inch photographic instrument with collimators for the Observatory of Georgetown College, and a four-and-one-half-inch meridian circle for the Catholic University. Numerous other important instruments are in course of construction. We hope this new and very powerful photographic transit instrument will enable F. Hagen and F. Fargis to carry their very promising experiments in the direction of determining right ascensions photographically to a successful conclusion. If it shall prove possible to photograph the collimators with success, there can be little doubt that most important results will flow from the use of this new method.

MESSRS. MACMILLAN & Co. announce that Dr. G. W. Hill's 'Celestial Mechanics' will be

published during the present year. The work will embody the lectures delivered at Columbia College by Dr. Hill, and will appear with the imprint of the Columbia University Press.

H. J.

HARVARD COLLEGE OBSERVATORY, CIRCULAR NO 5.

Wells' Algol Variable.

A MINIMUM of the Algol star, B. D. +17° 4367, occurred, as predicted in Circular No. 4, on the afternoon of January 5, 1896. Through the courtesy of Professor Young, observations were obtained at Princeton by Professor Taylor Reed, with the 23-inch equatorial. It was also observed by Mr. W. M. Reed at Andover. Preparations had been made at this observatory to obtain a series of photographic images of it automatically, each having an exposure of five minutes to observe it photometrically with the 15-inch equatorial, and also visually with the 12 and 6-inch equatorials. Unfortunately, owing to clouds, few observations were obtained, but these serve to show that the star was faint and diminishing in brightness as expected. Similar preparations were made for the next minimum, January 10, but again clouds prevented observation.

The observations so far obtained show that its time of minimum, uncorrected for the velocity of light, can be closely represented by the formula J. D. 2412002.500+4.8064 E. The uncertainty in the period does not exceed a few seconds, and will probably be known within a single second as soon as the form of light curve is determined. For nearly two hours before and after the minimum it is fainter than the twelfth magnitude. It is impossible, at present, to say how much fainter it becomes or whether it disappears entirely. It increases at first very rapidly and then more slowly, attaining its full brightness, magnitude 9.5, about five hours after the minimum. One hundred and thirty photographs indicate that during the four days between the successive minima it does not vary more than a few hundredths of a magnitude. The variation may be explained by assuming that the star revolves around a comparatively dark body and is totally eclipsed by it for two or three hours, the light at minimum, if any, being entirely that of the dark body. The conditions resemble those of U Cephei, which appears to be totally eclipsed by a relatively dark body two and a-half magnitudes fainter than itself, but having a diameter at least one half greater. The variation in light of B. D. +17° 4367 is more rapid than that of any other star hitherto discovered, and as its range is greater than that of any known star of the Algol type, its form of light curve can be determined with corresponding accuracy. U Cephei is second in both these respects.

The New Star in Centaurus.

In circular No. 4 insert 'it' before 'follows' in the ninth line. This word was given correctly in the printer's copy, but was omitted in setting the type. The correction was telegraphed to those astronomers who, it was expected, would use it. The Nova follows the nebula N. G. C. 5253, and is north of it. The nebula is assumed to be C. DM. —31° 10536, magn. 9.5, with which it was originally identified. As seen with a low power the nebula cannot readily be distinguished from a star. Its magnitude on the Cordoba scale by comparison with adjacent stars was estimated by Mr. Wendell as 9.7, and it could hardly have been overlooked in preparing the Cordoba Durchmusterung, in which many adjacent fainter stars are given. The new star could not have been observed at Cordoba unless we assume, first, that it was bright at that time, although invariably too faint to be photographed on fifty nights distributed over six years, and secondly, that the nebula was overlooked at Cordoba while observing fainter objects in the same region. Even if we make these assumptions, the new star still falls in the same class as T Coronæ, which was observed in the northern Durchmusterung several years preceding its appearance as a new star.

The various positions of N. G. C. 5253 for 1875 are as follows:—

Dreyer's New General Catalogue R. A. = $13^h\ 32^m\ 51^s$ Dec. = —31° 0′.2

Cordoba Durchmusterung R. A. = $13^h\ 32^m\ 49^s.6$ Dec. = —31° 0′.3

Plate B 13965 R. A. = $13^h\ 32^m\ 50^s.2$ Dec. = —31° 0′ 23″

Plate B 14072 R. A. = $13^h\ 32^m\ 50^s.0$ Dec. = —31° 0′ 21″

The positions of the Nova derived from these plates differ from each other by only $0^s.1$ in right ascensions and $1''$ in declination. The mean position for 1875 is R. A. $= 13^h\ 32^m\ 51^s.8$ Dec. $= -30^\circ\ 59'\ 58''$. It will be noticed that according to these measures, the Nova follows N. G. C. 5253 by $1^s.7$, and is $24''$ north.

EDWARD C. PICKERING.

JANUARY 31, 1896.

GENERAL.

MR. MORRILL'S bill in the Senate appropriating $250,000 for the erection of an additional building for the U. S. National Museum will be reported favorably by the Committee on Public Buildings and Grounds. The bill provides for a fire-proof building 300 feet square, having two stories and a basement.

THE daily papers contain much discussion regarding a dispatch purporting to come from Irkutsk, Siberia, and stating that Dr Nansen has reached the North Pole, has found land there and is now returning.

THE herbarium bequeathed by the late John H. Redfield to the Philadelphia Academy of Natural Sciences will be sold and the money used for a Redfield fund for the Botanical Department of the Academy.

M. ROUCHÉ has been elected on the second ballot, by 33 votes as compared with 29 cast for M. Lauth, *Membre libre* of the Paris Academy of Sciences. M. Moissan has been elected President of the Paris Chemical Society.

A BILL for the preservation of the Palisades, ceding to the United States jurisdiction over that part of the Palisades which lies in the State of New York, has been passed by the Legislature and will be signed by Gov. Morton.

THE Imperial German Health Bureau has reported that aluminum is especially suitable for cooking utensils, as it does not communicate any poisonous salts such as may arise from the use of copper, tin and lead.

A CABLEGRAM states that Prof. Röntgen was expected to conduct experiments on the X-rays before the German Reichstag, and that the Reichstag would be asked to make an appropriation for further researches. The daily papers continue to publish long accounts of experiments on the Röntgen rays, chiefly noticeable for their repetitions and inaccuracies. It is probable that no scientific advance has been made beyond what is contained in Prof. Röntgen's own paper published in the last number of this journal. It is, however, worth noting that Prof. Röntgen in his paper makes no mention of the possible applications of his discovery to surgery or elsewhere, but lays special weight on the speculation, having no apparent relation to his experiments, that the rays may be longitudinal vibrations in the ether.

Nature states that Mr. F. E. Willey, of the Royal Gardens, Kew, has been appointed Curator of the newly-founded Botanic Station at Sierra Leone. Mr. J. M. Henry has retired from the post of Superintendent of the Baroda State Gardens. He was sent out from Kew in 1867, and after twelve years' service in Madras and Bengal was appointed to Baroda in November, 1879.

THE Prussian Budget recommends the appropriation of $7,500 for the maintenance of a control station for diphtheria serum in connection with the Institute for Infectious Diseases.

THE Bender hygienic laboratory, now being constructed in Albany, will be completed during the present year and will contain every requisite of bacteriological investigations.

A CABLE dispatch states that a large aërolite exploded above the city of Madrid at 9:30 A. M. to-day. There was a vivid glare of light and a loud report. Buildings were shaken and many windows were shattered. According to the officials of the Madrid Observatory the explosion occurred twenty miles above the earth.

THERE is now a bill before the New York Assembly repealing the law compelling the schools to include the study of alcohol and narcotics in conjunction with the studies of physiology and hygiene. The Board of Education of the city of New York has voted to support this bill, and it has the support of the leading philanthropists and educators. The law passed last year by the Legislature of the State of New York and the similar laws in other States are regarded by those best competent to judge as injudicious and injurious to the cause of temperance.

EFFORTS are now being made to have the Legislature arrange for the permanent continuance of the geological study of the State of Maryland by providing for a State Geological Survey. Prof. William Bullock Clark will be placed in charge.

PROF. H. MARSHALL WARD, professor of botany in the University of Cambridge, is giving a course of three lectures on 'Some Aspects of Modern Botany' at the Royal Institution. The course began on February 13th.

A COPY of Audubon's *Birds of North America* is offered for sale in New York for $1,800. It is said to be unused and in the original binding, while a large part of the edition of 100 copies has had the margins of the plates reduced in size by rebinding.

WE learn from the *British Medical Journal* that fire broke out in one of the rooms of the Laboratory of the Edinburgh Royal College of Physicians on the night of January 31st, and resulted in disastrous consequences. The apparatus and specimens in one room were entirely destroyed by fire, and as these specimens had been brought together after the labor of years; the loss is irreparable. Several other rooms and their contents, including the chemical room, were seriously damaged by smoke and water. Had the fire not broken out in a room on the top flat and at an outside wall the results might have been vastly more serious. As it is, much has been destroyed that can never be replaced, even had insurances existed to the full. The work of the laboratory has been greatly disorganized, and some considerable time must elapse before the new buildings between Forrest Road and Bristo street are ready for occupation.

DR. BARNES has been elected the next President of the British Medical Association, which meets this year at Carlisle. Two addresses are to be given, one in Medicine by Sir Dyce Duckworth, and one in Surgery by Dr. Roderick Maclaren, and there are to be nine Sections, namely: Medicine, Surgery, Obstetrics, Public Health, Psychology, Pathology and Bacteriology, Ophthalmology, Diseases of Children, Medical Ethics.

PROF. BURDON SANDERSON has delivered a Friday evening lecture before the Royal Institution on Carl Ludwig and the mechanical physiology with which Ludwig's name is so closely identified. Prof. Sanderson said that the neo-vitalistic movement was already on the wane, and certainly that if any advance in knowledge is to be made the methods of research and reasoning adopted must be those of the Ludwig school.

THE Transactions of the American Microscopical Society, just published, contain a detailed account of the 18th annual meeting held at Cornell University last August, of which a report was given at the time in this journal. The next annual meeting of the Society will be held at Pittsburg, Pa., August 18, 19 and 20, 1896, under the presidency of Dr. A. Clifford Mercer, of Syracuse, New York.

HON. A. D. WHITE, formerly President of Cornell University, appeared on February 10th before the Senate Committee on a National University. He argued in favor of the plan, saying that in this respect the United Stated government is behind the European states. He contended that instead of weakening other universities, as had been claimed, the establishment of a National institution would strengthen all other seats of learning. It is expected that the committee will report favorably.

DR. DANIEL DENISON SLADE, lecturer on comparative osteology in Harvard University, and known for his contributions to osteology, zoölogy and botany, died at Chestnut Hill, Mass., on February 11th, aged 71 years.

MR. G. B. HOWES announces in *Nature* for January 23d, the discovery by Mr. J. P. Hill that the Bandicoot, *Perameles obesula*, possesses a true allantoic and highly vascular placenta of a discordal and most probably deciduous type. This, taken in connection with what is known to occur in *Phascolaretus*, weakens the line of demarcation between the marsupials and other mammals, or rather causes a slight overlapping of the two groups.

THE contents of the last issue of the *Bulletin of the Johns Hopkins Hospital* are very different from what most people would expect to find in a medical journal. There are three papers read by Prof. William Osler and one read by Mr. W.

B. Platt before the historical club of the hospital, and the address of Prof. W. H. Welch at the opening of the William Pepper laboratory of clinical medicine. The papers are all noteworthy for historical research and literary form. Prof. Osler reviews the life of Thomas Dover, physician and buccaneer, whose career throws curious light on the social conditions and medical practice in England at the beginning of the eighteenth century. In a second paper there is given from private sources an account of the life of an Alabama student, John I. Basset, "whose name was not written on the scroll of fame, but who heard the call and forsook all and followed his ideal." Prof. Osler's third paper is entitled 'John Keats, the Apothecary Poet.' Mr. Platt reviews the work of Johannes Müller as a physiologist and a teacher. Prof. Welch, in his address at Philadelphia, described the evolution of modern scientific laboratories. With the exception of anatomy, laboratories for instruction and research are comparatively recent. Purkinje's physiological laboratory at Breslau was established in 1824, one year earlier than Liebig's famous chemical laboratory at Giessen. Lord Kelvin established a physical laboratory in Glasgow about 1845. The first pathological laboratory was founded by Virschow, in Berlin, in 1856.

THE Division of Botany of the U. S. Department of Agriculture has issued a bulletin by Mr. L. H. Dewey reviewing the legislation undertaken by twenty-five of the States and Territories for the suppression of weeds and giving the essential provisions of a general State weed law.

THE Canadian government proposes to send an expedition to Hudson's Bay next summer to establish customs officers and to further investigate the navigability of Hudson's Straits.

THE position of scientific adviser to the London Trinity House, which has been in abeyance since the resignation of Tyndall, has been revived and has been accepted by Lord Rayleigh.

THE Royal Academy of Sciences of Belgium proposes, as the subject for a prize in 1897, a discussion from a theoretical point of view of the Variation of Latitude, its cause and meaning, together with a criticism of the works of geometers on the subject, from Laplace to the present time. A gold medal valued at 800 fr. will be awarded.

THE *London Times* states that investigations have recently been undertaken by the Marine Biological Association into the contents of certain bays on the south coast of Devon. The bays selected for the investigations were Start and Teignmouth Bays, both of which are closed to trawlers in accordance with a by-law of the Devon Sea Fisheries Committee. The object in view of which the work was begun was to discover the characteristic features of the localities in question in respect to the food fish they contained. Mr. F. B. Stead, the naturalist in charge of these investigations, has conducted trawling experiments in these localities during the months of October to December, and the most important facts ascertained by him are as follows: Of the different species of fish captured in the bays, plaice and dabs are by far the most numerous, and as of these two species the plaice is, from the economic point of view, far the most important, and the large number of competing dabs must probably be regarded as a positive hindrance to the well-being of the plaice, any controversy that may be raised as to the advisibility or otherwise of maintaining the by-law now in force should be solely occupied with the consideration of the question whether the closure of the bays to trawlers is necessary or desirable for the protection of the plaice. It has been further shown that the bays differ markedly from one another in respect to the sizes of the fish they contain. Thus, while half the plaice in Start Bay were found to be over 12½ in. in length, in Teignmouth Bay half the plaice captured were under 10½ in. A similar difference held in the case of the dabs. A preliminary account of these investigations will appear in the ensuing number of the journal of the Association.

UNIVERSITY AND EDUCATIONAL NEWS.

MRS. D. G. ORMSBY, of Milwaukee, has given $25,000 to Lawrence University at Appleton, Wis., to endow the 'D. G. Ormsby professorship of history and political economy,' in memory of the husband; and by the will of the

values of the astronomical constants were corrected, and in the rigor with which the entire discussion was carried through and the results presented.

A year or two after the appearance of Boss's work, the new system for the *Astronomische Gesellschaft*, constructed by Dr. Auwers, was published. A very slight examination of this work would show that its superiority to that of Boss was at least open to question. The weakest point was that the proper motions depended entirely on the observations of Bradley with the old mural quadrant, which was known to be subject to errors the amount of which did not admit of determination. But this defect did not prevent the general adoption of the foreign system by American astronomers, even in the case where the other would have been most eminently appropriate, the official work of boundary surveys.

There is one final and conclusive arbiter of all questions concerning the accuracy of predicted motions in the heavens. This arbiter is subsequent observation. Let us wait a sufficient length of time and see on which system the positions of the stars are most accurately predicted. In certain features of the system and in certain regions of the heavens the two works differed so widely that a very few years of accurate observations would suffice to settle the question.

About twenty years have elapsed since the last observations on which either of the two works was based. Within that time four catalogues of stars have appeared, founded on observations made at the respective observatories of Pulkowa and Greenwich, prepared with all the refinements of recent science, and therefore superior to any before made. In these results, combined with such conclusions as can be drawn from the best previous observations, we have the basis of a comparison which is found in the number of the *Astronomical Journal* quoted in the note found in the last number of SCIENCE. Without going into technical details, it will suffice to say that there are six separate and independent features in which the respective systems differed most largely. These six features, tested by the four modern authorities just quoted, showed the following average errors or difference between Boss's prediction and observations in different regions of the heavens, near the epoch of 1880:

$-0''.02$	$+0.02$
-0.03	$+0.02$
$+0.03$	0.00

It was then shown that, carrying back these six special points of difference between the two catalogues to the epoch of Bradley's observations, the actual differences between the two were larger than any likely deviation of Boss from the truth. In the most marked case the difference consisted in ten discrepancies, all in the same direction. Another very marked instance occurs in a region of the heavens including the northern part of the constellation Andromeda. In this region were found ten stars in the A. G. catalogue. The Polkowa catalogue of 1895, the most carefully prepared that astronomy has yet had at its command, showed that every one of these ten stars was in error in the same direction, that direction being the same in which they differed from the Boss system, and by amounts which could not be reasonably attributed to errors of the Pulkowa observations.

One would suppose the conclusion so obvious as to need no statement and admit of no question. Fifteen years of the most refined observations show a continuing agreement of the Boss system with observations which is most extraordinary, and which cannot possibly be shared by the other. This evidence, however, fails to convince the writer of the note. He claims that the results 'throw no new light on the subject.' If astronomers differ as to the question whether the approach to perfect agreement with observation above shown is conclusive, the question would seem to be forever incapable of decision.

Again, in the case of ten separate stars in which the deviations of the Bradley observations were all in the same direction, the writer remarks: "So we can hardly escape the conviction that our whole conclusion may be vitiated by a large error in a particular star."

Here it would seem that the astronomers must have recourse to legal advice to settle their dispute. Only a member of the legal profession can decide whether the concurrent evidence of ten

late Horatio Stone, Rockford College, Rockford, Ill., receives $28,000. Donations to the University of Pennsylvania during the past month amount to $69,370.23.

AT the meeting of the Board of Trustees of Princeton College, held on February 13th, Mr. J. Bayard Henry, '77, of Philadelphia, was elected trustee in place of William Libbey, of New York City, deceased, and Mr. Howard Crosby Warren, '89, was appointed assistant professor in experimental psychology.

ON the birthday of Mr. Henry W. Sage, celebrated at Cornell University on January 30, the following list of his gifts to the University was noted:

Sage College for women, with endowment fund (1873)	$266,000
Sage Chapel (1873)	30,000
Contribution towards extinguishment of a floating debt (1881)	30,000
House of Sage professor of philosophy (1886)	11,000
Susan Linn Sage chair of philosophy (1886)	50,000
Susan Linn school of philosophy (1886)	200,000
University library building (1891)	260,000
University library endowment (1891)	300,000
Casts for archæological museum (1891)	8,000
	$1,155,000

A MEMORIAL praying for the admission of women to degrees at Cambridge University has received the signatures of 2,200 university members.

DR. CESARE LOMBROSO has been transferred from the chair of legal medicine in the University of Turin, to the post of professor of psychiatry. He has also been made director of the University Clinic for Mental Diseases.

WILLIAM WARDE FOWLER, M.A., Fellow of Lincoln College, Oxford, has been appointed a Curator of the Botanic Garden, in place of Edward Chapman, M.A., Fellow of Magdalen College, resigned.

CORRESPONDENCE.

AMERICAN JUDGMENTS OF AMERICAN ASTRONOMY.

THE astronomical notes published in the last two numbers of SCIENCE afford instructive illustrations of a habit of judging American and foreign scientific work which is too prevalent among us. While in nearly every other country scientific investigators and writers are apt to be more or less biased in favor of their own countrymen, giving frequent occasion for remarks on their ignorance of what is going on outside and on their general insularity, the system prevalent among us is directly the contrary, at least in astronomy, and, to a certain extent, in the allied sciences. The way in which this bias displays itself is so well illustrated by the notes in question that we may be pardoned for taking them as a text for some remarks.

Among the great wants of astronomy for half a century past has been a standard system of positions of the principal fixed stars, which should serve as points of reference in defining the positions of other stars and of the heavenly bodies in general. The first step toward this end was taken by Dr. Auwers about 1870, and consisted of a determination of the corrections necessary to reduce the principal modern catalogues of stars to a homogeneous mean system; that is to say, to a system which should be as nearly as possible self-consistent, and express the mean result of all the determinations of positions made in each region of the heavens. But this work, though most ably performed and marking an epoch in astronomy of precision, was defective in not rigorously taking account of the proper motions of the stars. Hence, Dr. Auwer's system was valid only near a central epoch, say about 1840 or 1850. That he did not make it permanently valid was doubtless due to the fact that at that time the older observations, especially those of Bradley, had not been reduced with sufficient rigor to determine the proper motions. It was, therefore, a fitting complement of his work that he set about the thorough re-reduction of Bradley's observations at Greenwich with the mural quadrant, during the years 1750-1757.

About 1878 was published Boss's system of declinations, which appeared in a quarto volume of some 200 pages. A careful examination of this work showed that it stood unequalled in the thoroughness with which all the material was collected and worked up; in the completeness with which the errors of the older adopted

erence. But no one ever did or ever will count a group of horses, for instance, by first conceiving of an artificial * unit horse and then matching it with each actual horse in turn—which 'measuring' the group of horses must mean if it means anything."

The whole point here is under what circumstances does one, not a mathematician or for mathematical purposes, count a group of horses. The answer is something of the following sort, it seems to me: One counts when one wishes to find out how many horses he has caught in a day's hunt, whether the same number has been driven back at night that were taken out in the morning; how much money is to be got in selling them, it having been settled that each horse is to fetch the same sum, etc., etc.; how one ranks as a chieftain, or a soldier, compared with others, etc., etc. In other words, one not having arrived at the *abstract* interest of the mathematician (and certainly the child to be educated has not) counts only *when* there is some value to be ascertained, and counts *by* setting off something which, for present purposes is a sample unit of value, *e. g.*, a horse, then equating the total value to the number of such units. Taking the matter in its development then, (and not at the stage of the mathematician when abstracts have already become concretes) enumeration is always to define value, *i. e.*, to measure.

If the book referred to did not recognize the distinction between this sort of measuring and the technical sort it should certainly be condemned. But one of the points emphasized is that the former is an imperfect sort of measurement; that we don't really know, *e. g.*, what the possession of 60 horses amounts to till we know what one horse is worth, and so measuring proper (measuring with measured units) is substituted for mere counting, *i. e.*, measuring with undefined units of value.

2. It is said that number is not ratio. If one is using ratio to denote a certain idea, and not a technical abstraction of the mathematicians, I do not see how this statement is to be reconciled with Prof. Fine's own account of enumeration: "To count a group of things on the fingers is merely by assigning one of the fingers to each one of the things to form a group of fingers which stand in a *relation of 'one-to-one correspondence to the group of things.'*" * And again, "When we say of two groups of things that they are equal numerically, we simply mean that for each in the second there is one in the first, and for each thing in the first there is one in the second, in other words that the groups may be brought into a *relation of one-to-one correspondence.*" What does the phrase italicized mean, save the idea of ratio? If this way of stating it had only been known to me when the book reviewed was written, I should gladly have utilized it to indicate precisely the point we were trying to make—the implicit presence of the ratio idea in every number.

Psychologically there is, of course, a difference in the mental attitude in recognizing a thing as 'one,' as unity, as a whole, an individual, and recognizing it as '*a* one,' a unit. The primary problem the educator has to face, if he is to rationalize the teaching of arithmetic, is the discovery of this difference. The answer given is that 'one' (qualitative individuality or unity) becomes '*a* one,' a unit when it is *used* to *measure* value; and that, in turn, the need for this use arises when the thing is no longer taken as an adequate end, but as a means to be adjusted to some further end. *E. g.*, once more, when a man is *wholly* occupied in riding or hunting, or feeding a horse, when that absorbs his whole interest, he never takes the numerical view; when he wants to know how much of a horse owner he is, and how far this horse contributes to that end, he necessarily takes it. The question then is whether 'one' ever becomes '*a* one,' save as it is put into a 'relation of one-to-one correspondence?'

3. Prof. Fine remarks that 'the one postulate of arithmetic is that distinct things exist.' The mathematician may perhaps be reminded that this postulate is precisely one of the chief problems of the psychologist. Given a certain num-

* Whence and wherefore this artificial? The point to be proved involves nothing about an 'artificial' unit, but only a unit of reference, and that surely a horse is. But even if the term were relevant in the argument the question would arise whether the use of an artificial unit or of a measured unit is the essence of technical measurement; whether, indeed, a foot is, psychologically, more artificial than a horse.

* Italics mine.

independent witnesses, all testifying to the same fact, may be 'vitiated' by one of them being very much mistaken. It is to be regretted that the writer of the note does not tell us just how far the one erroneous star must have been wrong in order to vitiate the result. The corresponding testimony of the ten Pulkowa observations upon another group of ten stars may be left out of consideration, because this conclusion might be vitiated in the same way. S. NEWCOMB.

THE PERTURBATIONS OF 70 OPHIUCHI.

PROF. JACOBY'S review in a recent number of this journal (p. 197) is eminently fair in spirit; it is incomplete, and therefore I fear it will be misleading. It is a mistake to say that my work on the perturbations of 70 Ophiuchi is supported by the American observations, but contradicted by those made at the same time in Europe. On the contrary, the deviation from Schur's orbit and the work of the American observers is confirmed by the measures of all the best observers abroad. Thus the deviation appears unmistakably in the observations of Bigourdan, Callandreau, Schiaparelli, Glasenapp and Knorre. Since publishing the paper in *American Journal* 363, measures have been received from several of the above observers, and there is absolutely no doubt of the substantial accuracy of the American observations. Among the European observers Schur and Ebell (a student at Berlin) alone find no deviation, but Schur's measures are very discordant, and he admits (A. N. 3324) that they are of little value; while Ebell's measures show discrepancies on the several nights amounting to over ten degrees in angle.

Hence it is evident that all the best observations, both American and European, confirm the deviation from Schur's orbit and point to the existence of the dark body as the cause of this unexpected phenomenon. My researches on the orbits of 40 binary stars, which are now practically complete, will probably remove all doubt as to the propriety of using the distances in such investigations. Indeed the discovery of the perturbations in 70 Ophiuchi by using both angles and distances, after Schur had consciously rejected the distances which would have given him the discovery, is a striking illustration of the evil of orthodoxy in scientific procedure. T. J. J. SEE.

THE UNIVERSITY OF CHICAGO, February 11, 1896.

PSYCHOLOGY OF NUMBER.

TO THE EDITOR OF SCIENCE—*Sir:* As Prof. Fine in his review of McLellan's and Dewey's *Psychology of Number* (January 24, 1896) raised a question of considerable importance to educators and to psychologists, permit me to add a few words to the discussion, first thanking the reviewer for the generally appreciative tone of his article.

1. The question of principle raised is whether or no counting is measuring, whether or no integral number has a metric origin or purpose, and involves the idea of ratio. Now measurement is a word both of a more general and a more technical sense. That, in the most technical mathematical sense, counting is not measurement, is clearly recognized in the book referred to. But as it is held that in the larger sense of the term it is a process of measuring, and that the technical mode of measurement is an outgrowth, psychologically, of the broader and looser sense, this disclaimer amounts, perhaps, to little.

Starting from the larger sense, it is held that number has its psychological genesis in the felt need for valuation, and that its function (psychologically once more) is to serve the purposes of valuation. Now counting seems to me indubitably one mode of defining the value of a previously unvalued mental whole, and in that sense to be a mode of measurement. Any process of defining value is, I should say, a form of measurement in the broad sense of that term. Counting implies first a mental whole; secondly, the breaking up of that whole into distinct parts; third, the use of one (any one, not some one) of these parts as a unit; fourth, the measurement of the amount or value of the original whole, through equalizing it to a certain definite number of the selected unit.

But Prof. Fine says: "In however loose a sense the word may be used, 'measuring' at least involves the conscious use of a unit of ref-

ber of things already recognized as distinct, and it is a very simple matter to go ahead and enumerate them, though even that must have a psychology *motivation*. But the whole tendency of contemporary psychology is to take a psychical continuum as its datum, and find distinctness (the property at the basis of number) as the outcome of a process of differentiation. The identification of this process, the ascertaining of the circumstances under which it arises, the mode of its operation—this is the thing which the psychologist wants to know about number, and is the thing the educator must know to secure the conditions under which the child shall form the number concepts easily and efficiently. The theory of the book, 'Psychology of Number,' viz., that the differentiation and enumeration of units arises through the progressively accurate adjustment of means to end, may be right or wrong, but its error can hardly be established, I take it, by a mathematical view which considers number only as it is after it is fully developed, and has become so familiar as to be itself a complete object to the mind. Without pretending to a knowledge of numerical theory which I do not possess, I may say that it seems to me that the work done by Gauss is at precisely the opposite pole from that which the educator needs from the psychologist, *i. e.*, Gauss was attempting to reduce to its ultimate simple numerical generalizations the developed mathematical structure. Dr. McLellan and myself were engaged upon the much humbler task of finding out what sort of a mental condition creates a demand for number, and how it is that number operates to satisfy that demand.

May I conclude by referring to the practical point involved? The trained mathematician as such is, of necessity, interested in the further use of certain finished psychical products. As a mathematician any reference to the preliminary development of these products can only disturb and divert him. But the problem for the pupil *is how to get the standpoint of the mathematician;* not how to use certain tools, but how to make them; not how to carry further the manipulation of certain data, but how to get meaning into the data. This is ultimately a psychological question, not a mathematical one, although it has to be translated over into mathematical terms and processes; and none is so well fitted to do it as the mathematician, provided only he will project himself far enough backward in the scale of development to realize the problem. The point does not conclude with primary instruction. Our text-books of algebra, geometry and high analysis are almost entirely written from the standpoint of an elegant and logical exposition of the matter as it stands to the trained mathematician. They are very nice for one who doesn't need them any longer. The first books written from the standpoint of one who is still coming to consciousness of the meaning of his concepts will, perhaps, seem foolishness to the trained mathematician, but they will mark the dawn of a new day to the average student. I venture the statement that (putting aside the few with the inborn mathematical instinct) higher and secondary mathematics is to the majority of students a practical riddle with no definite *intellectual* content in itself. What meaning it possesses it has got by way of attained practical facility in solving problems; or through its applications to other sciences or to engineering. It will hardly be denied that the educational value of mathematics is not realized until its concepts and methods have a definite intellectual meaning and content of their own. Can this be secured, save as the methods of instruction follow the evolution of the process out of its cruder psychical forms to the more finished?

I shall be more than satisfied to have made many blunders on the mathematical side if only I do not offer myself up in vain as a spectacle; if only more competent psychologists take up the matter, and if only mathematicians may descend from their acquired mathematical plane and endeavor to rethink the psychical conditions and steps through which their present magnificent apparatus has grown out of primitive, non-mathematical or crudely mathematical forms up to its present high estate. If the psychologist will risk some blundering around among the mathematical concepts, and the mathematician will recognize the relevancy of the psychological demand, and venture a little blundering upon that side, both parties may not only come to an understanding, but mathematical teaching may get what it to-

day so largely lacks, some relationship to the psychical needs and attitudes of those under instruction. JOHN DEWEY.

UNIVERSITY OF CHICAGO, February 6, 1896.

DOES THE PRIVATE COLLECTOR MAKE THE BEST MUSEUM ADMINISTRATOR?

THE concluding portion, section K, of Dr. Goode's recent paper on the Classification of Museums, is devoted to a consideration of private cabinets and collectors, and to the major portion of the propositions therein laid down all can heartily subscribe. There is, however, one among them to which I can not fully assent, at least so far as museums of natural history are concerned, and that is, that "The person who has formed a private collection can most successfully manage one for the use of the public."

It must be confessed that this doubt largely rests upon theory, but an acquaintance with some collectors makes it seem probable that it is, after all, well founded.

A considerable amount of collecting is done with no purpose in view other than that of accumulating specimens, but, on the other hand, a private collection may be formed with a definite purpose and along certain lines. In the one case the collector certainly shows no unusual fitness for a position in a museum, while in the other he is interested in his collection for what he can get out of it himself and not for the benefit it may be to others, and this is exactly the opposite view to that which should be held by an officer in a public museum. This is not saying that such is the point of view universally assumed by museum curators, but it is certain that the success of a public museum depends on the extent to which it is adopted. Again a private collector is, from the nature of the case, apt to be one-sided, to lay too much stress on one group to the exclusion of others, and thus to lack the evenness of balance which should be one of the characteristics of the 'museum man.' This one-sidedness frequently takes the form of undue preference for rare or costly specimens, attaching an undue importance to the specimens themselves rather than what is to be got out of them.

Moreover the care and arrangement of a private study series and of a public study series, and, above all of an exhibition series, are entirely different things and require a totally different treatment. A private series may be ill-arranged and poorly labeled, but the owner knows each specimen, its history and whereabouts. A public study series should, on the contrary, be so arranged and so labeled that any student may consult it and make notes upon it, while in an exhibition series the specimens should be so chosen that, while each conveys some information, all form a harmonious whole.

A private collector may know his own needs, but he would not know or would not care for the needs of the public, and he would carry to a public museum the taste for accumulation, or for research, which probably led to the formation of his own collection. Accumulation is a good thing, but it needs to be properly directed in order to be of public service, while there is probably no greater drawback to the public efficiency of a museum officer than too great devotion to original research, as this leads not only to lack of care for material which has served its turn, but to a very decided lack of interest in the public which must be reached through the exhibition series.

This criticism is by no means to be construed into a criticism of the private collector; the value of his work and the influence of his collections are immense; it is simply a denial of the proposition that because a man has formed a private cabinet he is therefore best fitted to administer a public museum.

F. A. LUCAS.

WASHINGTON, D. C.

SCIENTIFIC LITERATURE.

Lehrbuch der Entwicklungsgeschichte des Menschen und der Wirbelthiere. OSCAR HERTWIG. Jena, Gustav Fischer. 1895. Pp. xvi + 612.

This excellent work now appears in a fifth edition, in which many improvements have been made. Prof. Hertwig is especially distinguished both for his comprehension of the problems of morphology and for the lucidity of his explanations, so that his text-book has long been accepted as a valuable treatise both for students and for advanced workers, and has been accorded the distinction of translation into several languages. A very admirable

translation into English has been published by Prof. Mark, based upon the third German edition. The book has already an assured and high place, and is so well known that it is only necessary to state that its typography and general appearance have remained unchanged.

In the new edition many much needed improvements have been made, and several parts have been entirely recast to concord with the latest progress. The revision has touched especially the following parts: the problem of reduction-division; the rôle of the centrosome in impregnation, the development of the middle germ layer in reptiles and mammals, the structure of the chorion, the origin of striated muscles, of blood corpuscles and the development of the vesicula; and there is one entirely new section, which will be welcome to many embryologists and bears the title, 'Experiments and theories on the significance of the first-formed cleavage cells and of single parts of the ovum for the formation of the organs of the embryo.' There have been so many researches in this field, and they bear so directly on Weismann's and other theories of heredity, that the synopsis given by Hertwig will appeal both to those actively at work in this comparatively new region of embryo-mechanics, and to those who wish to learn what conclusions have been reached up to the present. We may also note that since the first edition the number of illustrations has risen from 304 to 384, and the number of pages from 507 to 612.

There are certain general criticisms to be made upon Hertwigs' text-book. It is certainly a defect that the author leans far too much upon both diagrammatic pictures and upon diagrammatic explanations, and does not allow free play to actual observation. This is disastrously the case in his sixth chapter in which he deals with the origin of the middle germ-layer (cœlom theory), and by artful shading misrepresents the actual facts in a manner which is inexcusable even in a text-book. Facts ought never to be mixed with error merely because such dilution serves to hide their discordance with the author's theoretical views. The same tendency to uphold his cœlom theory shows itself in another way in that he still entirely separates the mesenchymal tissues and the mesothelial (to which latter he erroneously restricts the term mesoblast, p. 115), although it was proven several years ago so as to be past doubt, that Hertwig's view was unjustified and that mesenchyma and mesothelium are parts of the same layer or mesoderm. This result is not a matter of opinion, it is simply a matter of direct observation. This conclusion Hertwig has admitted, yet fails to make it the basis of his exposition, and instead continues the unnatural separation of the two portions of the middle germ layer, much to the confusion of young students.

Other unfavorable criticisms may be made in regard to special parts of the subject treated inadequately. Such parts are: 1. The nervous system, in that he fails to bring out the fundamental division into dorsal and ventral zones, or the existence of the three primary layers of the medullary wall, or the significance of the neck-bend, or the history of the neuromeres. 2. The fact that the nails are modifications of the stratum lucidum of the epidermis, a very important morphological fact. 3. The development of smooth muscle. 4. The history of the group of connective tissues. 5. The account of the formation of the renal tubules is erroneous, and is the most serious defect noticed by the reviewer. 6. The origin and significance of the yolk cavity and its fusion with that of the notochordal canal in Anura and Amniota to form the definite entodermal canal is not discussed, yet it is a very important point in the morphology of the higher vertebrate embryos. These and other examples which might be given show that Hertwig is far from giving a well-rounded presentation of our present knowledge, and that very much needs to be added to make it a thorough and comprehensive treatise.

In spite of these limitations, Hertwig's Embryology is a text-book of the first class, and has done and will probably long continue to do much for the promotion of the branch of science with which it deals. The treatment of the subject is fresh, original, strong and well proportioned, so that the leading points receive due emphasis. In many parts Hertwig speaks with the highest authority, notably in regard to the earlier stages of development, and the history of the genital products. The illustrations are

admirably selected and well executed, except for their tendency toward schematization. The original figures are not numerous and are chiefly diagrams.

In conclusion, it may be said that any student who, with the aid of practical laboratory work, masters Hertwig's book will have mastered the general subject of human embryology from the comparative morphological standpoint, and will be qualified to pursue more advanced study, but he must remain ready to modify many of his general theories and to fill out a number of important gaps in his knowledge. His chief gain will be insight into the very spirit of morphology, through the guidance of one of the very ablest of morphologists.

C. S. MINOT.

A Handbook of the British Macro-Lepidoptera. By BERTRAM GEO. RYE. With hand-colored illustrations by MAUD HORMAN-FISHER. London, Ward & Foxlow. Parts 1-4, Jan.-Oct., 1895.

The four parts issued give a fair idea of the scope and execution of this addition to the already large number of works relating to the butterflies and moths of Great Britain. Each part contains eight pages and two plates.

In the introduction the changes that take place during metamorphosis and the principal characters used in classification are briefly described. Eight families of Rhopalocera are recognized, namely, Papilionidæ, Pieridæ, Nymphalidæ, Apaturidæ, Satyridæ, Lycænidæ, Erycinidæ and Hesperidæ. A table separating these is given, and the genera and species can be readily distinguished by means of similar tables. The species are fairly well described, and the notes on the early stages, haunts, times of appearance, and abundance are clear and concise.

The plates are excellent, and the distinctive value of Mr. Rye's work consists in the description and illustration of the varieties and local races, apart from the consideration of the species, of the Macro-Lepidoptera of Great Britain. Beginning with 1896 the parts will be issued bi-monthly, instead of quarterly. The price per part is 2s. 6d.

SAMUEL HENSHAW.

Mollusca and Crustacea of the Miocene Formations of New Jersey. By R. P. WHITFIELD. Monograph U. S. Geol. Survey. Vol. XXIV. 1894.

This latest contribution of Professor Whitfield to the paleontology of New Jersey is most opportune, since the detailed mapping of the coastal plain formations of the State has recently shown an extensive development of Miocene strata. The character of the deposits is such, however, that determinable fossils have only been detected at a very few points, the great majority coming from the marl beds in the vicinity of Shiloh and Jericho and from the deep well-borings at Atlantic City. These forms Prof. Whitfield has evidently studied with great care and has presented in a most acceptable manner.

Prior to the publication of this report by Prof. Whitfield, little systematic work had been done upon the fossils of the Miocene of New Jersey. Meek's list, published in the 'Smithsonian Miscellaneous Collections' in 1864, contains reference to only seventeen species. Prof. Heilprin in his 'Tertiary Geology of the eastern and southern United States,' published in 1884, gives twenty-seven species, seventeen of which he regards as peculiar to the State. Later, from time to time, the same author added to this list, until in 1887, in an article on 'The Miocene Mollusca of the State of New Jersey,' he enumerates eighty-two species, describing three new species and one variety.

In his monograph Prof. Whitfield recognizes one hundred and four species, but states that there is no doubt that many more species might be obtained were the beds more thoroughly examined and other localities explored. Of the species described thirty-six are regarded as peculiar to New Jersey.

Besides the molluscan remains enumerated, Mr. Anthony Woodward gives a list of twelve species of foraminifera found in the marls at Shiloh and two at Jericho.

Prof. Whitfield, from a study of the fossils, would correlate the deposits with the Miocene of the States to the south, which is fully substantiated upon physical grounds as well. The writer of this review has traced the strata across Delaware into Maryland so that there can be no doubt but that the New Jersey Miocene is

the direct continuation northward of the Chesapeake formation of the Middle Atlantic slope.

W. B. Clark.

SOCIETIES AND ACADEMIES.

THE PHILOSOPHICAL SOCIETY OF WASHINGTON, FEBRUARY 1.

Mr. Lester F. Ward read a paper on 'The Filiation of the Sciences.' The purpose of the paper was to trace the progress of the conception of a natural order of development for the larger groups of phenomena, as distinguished, on the one hand, from any attempt at a logical classification of the sciences, and on the other, from the consideration of the order in which the sciences have been historically developed. Without going back of the present century to deal with the more or less fanciful notions of the Ancients or of such moderns as Oken, Hegel, d'Alembert, Hobbes, Locke, etc., he drew attention to the views of Auguste Comte and Herbert Spencer, as the two philosophers who had clearly conceived the problem of natural evolution.

He first traced the development of the idea in the mind of the first of these writers from 1820 to 1842. In a paper published by him in 1820, he had quite clearly expressed the fundamental truth, and arranged the great groups of phenomena, or sciences, in the following order: 1, Mathematics; 2, Astronomy; 3, Physics; 4, Chemistry; 5, Physiology; giving to each of these terms a wide meaning, but admitting that mathematics was not coördinate with the others, but was only the criterion by which each of the others was to be judged and its position in the series fixed. From 1826 to 1829 he elaborated this scheme in a course of lectures, soon after published as his well-known work on Positive Philosophy, the first volume of which appeared in 1830. In the prospectus of these lectures, circulated in manuscript form in 1826, he added to the above five sciences a sixth, viz., Social Physics, and the scheme as then drawn up was introduced in tabular form at the beginning of the first volume of the Positive Philosophy. In Vol. III. of that work, which appeared in 1838, he substituted for his 'Physiology' Lamarck's term Biology, but the scope of this science was the same as before and practically that of biology as now understood. The last chapter of that volume was devoted to what he called the intellectual and moral, or cerebral, functions of life, in which he fully recognized the present science of psychology, but denied that it could be properly separated from biology. In the fourth volume, published in 1839, he speaks of this as 'Transcendental Biology.' It is in this volume, too, that he first proposed the term 'Sociology,' as the exact equivalent of his 'Social Physics,' and continued to the end to use both these terms interchangeably. It was not till 1842, with the appearance of the first volume of his Positive Polity (*Politique Positive*), that he added anything to the scheme of sciences thus drawn up. He then recognized, as the seventh and last term of the series, the science of Ethics. The entire series, then, as he finally left it, was as follows: 1, Mathematics; 2, Astronomy; 3, Physics; 4, Chemistry; 5, Biology (including cerebral or transcendental biology); 6, Sociology; 7, Ethics.

Comte was at great pains to explain that this series represented the true order of nature, and that the phenomena corresponded to the actual evolution that has taken place in the universe. The degree of 'positivity' of any science is that to which it can be reduced to mathematical laws. The first of the sciences that represent phenomena, viz., astronomy (from which sidereal astronomy was excluded) is therefore the most positive, and the degree of positivity diminishes with each term in the series. The sciences thus arranged also diminish in their generality while they increase in their complexity.

Moreover, each higher science has its roots in the one next below it and is, as it were, derived from it. The relationship is genetic, and hence his favorite term 'filiation,' a word much better chosen than the term 'hierarchy' which he also applied to the system.

Mr. Ward next proceeded to consider the scheme of Mr. Herbert Spencer as elaborated in his Synthetic Philosophy. A prospectus of that work was circulated in 1860. It was to embrace one volume on First Principles, two volumes on the Principles of Biology, two volumes on the Principles of Psychology, three volumes on the Principles of Sociology, and two volumes on the Principles of Morality. In this pro-

spectus, between the First Principles and the Principles of Biology, was inserted the following explanatory note: "In logical order should here come the application of these First Principles to Inorganic Nature. But this great division it is proposed to pass over, partly because even without it the scheme is too extensive, and partly because the interpretation of Organic Nature after the proposed method is of more immediate importance." This scheme of course was regarded by all as representing Mr. Spencer's conception of the natural order of evolution in the universe, and the arrangement of his topics was supposed to reflect his views of the actual succession of cosmic events. The groups of phenomena, *i. e.*, the several great sciences, would, therefore, stand as follows:

1. Inorganic Nature (subdivisions not indicated); 2. Biology; 3. Psychology; 4. Sociology; 5. Morality. How closely he has adhered to this scheme is known to all, the only deviation being the merely verbal one of substituting the word Ethics for 'Morality' in the title of the last work.

How he would have subdivided the phenomena of inorganic nature, and how he would have designated and arranged the subdivisions, has remained for the most part a matter of inference. In illustrating the cosmical laws laid down in his First Principles he frequently swept across the whole field and generally began with the nebular hypothesis and astronomical phenomena, then dealt with planetary and terrestrial events, involving the action of heat, light, electricity, etc., and passed to organic phenomena through the chemical process by which the higher compounds have been developed. From this it was inferred by some that his arrangement of the inorganic sciences, had he worked it out, would have been the same as Comte's, viz: Astronomy, Physics, Chemistry.

In 1864 he published his Classification of the Sciences, but even here this question was not answered to the clear comprehension of all, for a classification may be quite a different thing from a genesis or filiation of the groups of phenomena classified. Still, inasmuch as he classed physics and chemistry as 'abstract-concrete' sciences, dealing with the 'elements' of phenomena, while astronomy, geology, biology, psychology and sociology were classed as 'concrete' sciences, dealing with the 'totalities' of phenomena, it was safe to assume that it was to the latter group alone that he proposed to confine his Synthetic Philosophy; and in the larger table of the concrete sciences, after making astronomy coördinate with the combined phenomena of 'astrogeny' and 'geogeny,' he arranged under the last of these groups, biology and the other organic sciences in a scale of progressive subordination.

In an article dated December 3, 1868, and published as an appendix to the first volume of his Principles of Biology (not, of course, to the first edition, which appeared in 1867), he says; "I am placed at a disadvantage in having had to omit that part of the System of Philosophy which deals with Inorganic Evolution * * * which should * * * precede the *Principles of Biology*. Two volumes are missing. The closing chapter of the second, were it written, would deal with the evolution of organic matter —the step preceding the evolution of organic forms;" and he then proceeds to discuss this aspect of the subject in connection with the doctrine of spontaneous generation, respecting which he had been misunderstood by his critics. He deals with it mainly from the chemical standpoint, as, indeed, he also does in the opening chapters of that volume.

Once more, at the very beginning of his Principles of Sociology, the first part of which appeared in 1874, he remarks: "Of the three broadly distinguished kinds of Evolution, we come now to the third. The first kind, Inorganic Evolution, which, had it been dealt with, would have occupied two volumes, one dealing with Astrogeny and the other with Geogeny, was passed over, etc." This would seem to leave no further doubt upon the point in question.

Mr. Ward added that he had recently received a letter from Mr. Spencer in which the series was given complete according to his present view of the subject, and in which he admitted that he had aimed to confine the treatment in the Synthetic Philosophy exclusively to the concrete sciences as defined in his Classification of the Sciences. This latest version of the matter is given in the right-hand column of

the following table, the final arrangement of Comte being shown in the left-hand column for porposes of comparison:

System of Auguste Comte.	System of Herbert Spencer.
1. Astronomy.	1. Astronomy.
2. Physics. }	2. Geology.
3. Chemistry. }	
4. Biology (including	3. Biology.
5. CerebralBiology).	4. Psychology.
6. Sociology.	5. Sociology.
7. Ethics.	6. Ethics.

Mr. Ward said that he would himself agree with Spencer in admitting psychology to equal rank with the other members of the series, but that he would differ from both Comte and Spencer in assigning such rank to ethics, which he regarded a subdivision of sociology.

When it is remembered that the question involved is solely that of the natural order of evolution, or genesis of the successive groups of phenomena, and not that of the logical relationships of the sciences that have to deal with them, still less that of the historical order in which these sciences have been cultivated, it seems clear that it makes little difference whether, with Comte, the attention is concentrated more upon the laws governing the phenomena, or, with Spencer, upon the objects manifesting the phenomena. The series is virtually the same in either case, and it may be fairly claimed that it embodies the largest truth which the universe presents.

Mr. Ward's paper was discussed by Mr. J. W. Powell and Mr. Henry Farquhar.

W. C. Winlock,
Secretary.

ENTOMOLOGICAL SOCIETY OF WASHINGTON.

The 114th regular meeting was held February 6th. Mr. Schwarz read a communication on the 'Sleeping Trees of Hymenoptera in Southwestern Texas.' Sleeping specimens of two species of Apidæ, *Melissodes pygmæus* and *Coelioxys texana* could frequently be seen near San Diego, Texas, in the early morning hours on the thinnest twigs and thorns of dead bushes of *Celtis pallida.* The sleeping bees hold the twig or thorn firmly grasped with all six legs, and further secure their position by inserting the tips of the widely separated mandibles firmly into the wood. Certain bushes of rather small size are selected by the bees as common sleeping quarters, and on such bushes the two Apidæ are always associated with a Sphegid, *Coloptera wrightii.* The similarity of these sleeping quarters with the so-called 'Butterfly' trees, which are the common sleeping places of *Danais archippus* was discussed.

The paper was discussed by Messrs. Howard, Ashmead, Benton, Gill, Stiles and Fernow. Mr. Ashmead had little doubt of the entire novelty of the observations. Mr. Benton described the position of the honeybee when asleep. Drs. Gill and Stiles and Mr. Fernow discussed the question of sleep and rest with other animals.

Mr. Howard read a paper on the transformations of *Pulex serraticeps*, showing that the common household flea, to which so much attention has been attracted during the past few summers in Northeastern cities, is this common cosmopolitan pest of the cat and dog. He gave the results of careful observations made upon different stages of the insect, and showed that the entire life round from the egg to the adult may occupy in the summer at Washington but sixteen days, the transformations being as rapid as at Calcutta, India. This paper was discussed by Messrs. Patten, Fernow, Barnard, Schwarz, Benton, Ashmead, Marlatt and Gill, who told many stories of the habits and ferocity of fleas in different parts of the world.

L. O. Howard,
Recording Secretary.

GEOLOGICAL SOCIETY OF WASHINGTON.

At the forty-second meeting of the Geological Society of Washington, held on Wednesday, January 29, 1896, Prof. C. R. Van Hise, of the University of Wisconsin, presented a communication on the Relations of Primary and Secondary Structures in Rocks, being a continuation of the subject considered at the preceding meeting.

The relations between cleavage and fissility were discussed. It was concluded that fissility in many cases is controlled in its direction by a previously developed cleavage. Further, most rocks, at the surface having the property of cleavage which developed under deep seated

conditions, show, to a greater or lesser degree, a fissility developed when they were nearer the surface.

The relations of the secondary structures, cleavage and fissility, to bedding were considered. It was shown that there is a tendency for the primary and secondary structures to become parallel or nearly so on the limbs of the folds and to intersect each other at the arches and troughs. In case the folding is close the two structures may be so nearly parallel, except at the short turns of the anticlines and synclines, that the fact that there is a discrepancy anywhere is likely to be overlooked and the conclusion reached that in a given district the two structures are everywhere accordant. This mistake in the past has frequently led to great overestimates of the thickness of formations having slatiness or schistosity.

Prof. Van Hise's observations and conclusions were corroborated and supported by Messrs. Diller, Willis and Keith.

W. F. MORSELL.

NATIONAL GEOGRAPHIC SOCIETY.

AT the regular meeting of the National Geographic Society, held in Washington, D. C., February 7, 1896, Prof. W J McGee, of the Bureau of American Ethnology, presented a communication on 'A Sojourn in Seriland,' which was illustrated by lantern slides. The paper was an account of his recent explorations among a hostile, savage and little known people near the Gulf of California. Mr. McGee gave a brief sketch of the country traversed, with special reference to the flora and fauna and the characteristics of the Seri Indians. A feature of the address was a description of thirst, real and extreme thirst, based on the experience and observation of the speaker.

At the lecture on February 14th, Capt. Z. L. Tanner, United States Navy, commander of the United States Fish Commission Steamer Albatross, described the Commission's method of deep sea fishing, and the forms of submarine life brought up by the dragnet. He also described the voyage of the Albatross from the Atlantic to the Pacific, where she visited the Galapagos Islands and ran several lines of soundings for a submarine cable from California to the Hawaiian Islands. The lecture was illustrated by lantern-slide views of scenes both on shipboard and ashore.

W. F. MORSELL.

BOSTON SOCIETY OF NATURAL HISTORY.

A GENERAL meeting was held January 15th; eighty-four persons were present. The proposed By-Laws of the Society were first considered and, after discussion and acceptance of a single amendment, they were adopted.

Mr. William Brewster spoke on the natural history of Trinidad, illustrating his remarks with a series of lantern slides, showing views of the vegetation and of various animals. He sketched the general characters of the island, the temperature, climate, etc., and referred to the value of the government resthouses to travellers and naturalists. The fauna and flora of Trinidad is the same as that of the valley of the Orinoco; many of the birds and plants are identical with those found on the Amazon. The absence of annoying insects was especially noteworthy and the protective coloration of the birds universal. The forests with the scarcity of brilliantly colored animals, and the trees noticeable for the smallness of their leaves, gave a first impression not very different from that derived from a New England forest. Mr. Brewster read from his journal various notes on the characteristics and habits of some of the conspicuous mammals, birds, reptiles, and insects, noting especially the habits of the parasol ants and the fungus-hunting ants, and closed with a reference to the palatableness of the Agouti, Lape, Peccary and Howling Monkey.

SAMUEL HENSHAW,
Secretary.

THE TORREY BOTANICAL CLUB.

THE regular meeting of the Torrey Botanical Club was held on Wednesday evening, January 29th, with 38 persons in attendance. Ten new members were elected.

Dr. Valery Havard, U. S. A., read a very interesting paper entitled 'Drink Plants of the North American Indians.'

These plants were divided into three classes:

1st. Plants yielding alcoholic drinks.

Distillation was unknown to the North Ameri-

can aborigines, and their few alcoholic drinks were such as could be readily obtained by the fermentation of saccharine fluids.

In Mexico the two plants commonly used for these drinks were Maize and Maguey (*Agave Americana*), and, to a lesser extent, the fruit of *Opuntia Tuna*, *O. Ficus-Indica*, *Yucca baccata* and *Y. macrocarpa*.

In the United States the only Indians preparing alcoholic drinks were a few southwestern tribes; Apaches, Pimos, Maricopas, Papagos and Yumas, which probably obtained the knowledge from Mexican natives early in this century. The plants used were Maize (only by the Apaches) *Agave Parryi* and *A. Palmeri*, the pulpy fruit of the Pitahaya (*Cereus giganteus* and *C. Thurberi*) and the bean of the Mezquite (*Prosopis juliflora* and *P. pubescens*).

2d. Plants yielding stimulating, deliriant or intoxicating principles other than alcohol.

The Peyote (*Anhalonium Engelmanni* Lem.) and Mescal Buttons (*Lophophora Williamsii Lewinii* Coulter) of the Rio Grande and North Mexico, the Frijolillo (*Sophora secundiflora*) of Texas, several species of *Datura*, specially *D. meteloides*, and the Cassine or Yupon (*Ilex vomitoria*) of the southern Indians from which they prepared their favorite 'Black Drink.'

3d. Plants yielding palatable and nutritive sap or juice, or, by infusion, pleasant beverages or teas.

The saps most used were those of Maples (*Acer saccharum*, *A. saccharinum* and *A. rubrum*), and to a lesser extent that of Box Elder (*Acer negundo*), of the Butternut (*Juglans cinerea*) and of the Birch (*Betula lenta* and *lutea*).

The juicy plants of desert regions: Leaves and stems of several species of *Agave*, *Opuntia* and *Echinocactus*, the Sotol (*Dasylirion Texanum*) and the Sand Food (*Ammobroma Sonoroæ*).

Plants whose seeds were infused in water for their mucilage, sugar, oils, &c.: Maize, Mezquite and several species of Sage, chiefly *Salvia polystachya*, the Chia of Mexico, and *S. Columbariæ*, the Chia of California and Arizona.

Plants with tart fruit imparting a pleasant acidulous taste to water: Several species of Sumach on the Atlantic and Pacific coasts, the Manzanitas (*Arctostaphylos Manzanita* and *tomentosa*) of California, the Bulberry of the Missouri region (*Shepherdia argentea*), the Soapberry of the Northern States (*S. Canadensis*) and various species of Barberries (*Berberis*).

Plants containg mostly volatile oils, making agreeable, fragrant teas: Sassafras, Spice bush (*Benzoin Benzoin*), Wintergreen (*Gaultheria procumbens*), New Jersey Tea (*Ceanothus Americanus*), Labrador Tea (*Ledum Greenlandicum*), Sweet Goldenrod (*Solidago odora*), Pennyroyal (*Hedeoma pulegioides* and *Drummondi*), *Croton corymbulosus* and *suaveolens*.

Dr. John K. Small presented his 'Preliminary Notes on the North American Species of Saxifraga,' proposing to separate from that genus the two new genera Japsonia and Saxifragopsis.

Dr. N. L. Britton read a paper entitled 'New or Noteworthy species of Cyperaceae.' He proposed a number of new species, reduced two species and submitted a large number of valuable notes, especially on geographical distribution.

Dr. Britton also submitted observations and specimens in support of Pursh's *Lilium umbellatum*, a species which has been uniformly accepted in herbaria as *L. Philadelphicum*. This view was endorsed by Mr. Rydberg.

H. H. RUSBY,
Secretary.

NEW BOOKS.

Physiological Papers. By M. NEWELL MARTIN. Baltimore, Johns Hopkins Press. 1895. Pp. vii. +264.

Elements of the Theory of Functions of a Complex Variable. By DR. H. DURÈGE. Authorized translation from 4th German Edition. George Egbert Fisher and Isaac J. Schwatt. Philadelphia, G. E. Fisher and I. J. Schwatt. 1896. Pp. xiii. +288.

A Text-Book of Gas Manufacture for Students. JOHN HORNBY. London, George Bell & Sons. New York, Macmillan & Co. 1896. Pp. vii+261. $1.50.

Naturwissenschaftliche Einführung in die Bakteriologie. FERDINAND HUEPPE. Wiesbaden, C. W. Kreidel. 1896. Pp. viii. + 268. M. 6.

Die Lehre von den spezifischen Sinnesenergien. RUDOLF WEINMANN. Hamburg and Leipzig, Leopold Voss. 1895. Pp. 96. 1895. M. 2.25.